"Incongruent Bedfellows": Physics and Medicine in the Formation of North American and British Radiology, 1896-1930

by

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Abstract

The announcement of the discovery of x-rays in 1896 sparked great excitement among physicists who rushed to replicate Wilhelm Röntgen's experiments, hoping to make sense of the properties of this new disturbance in the aether. The medical world was equally entranced by the x-ray pictures that circulated in the press. The diagnostic possibilities were clear, and within a few months, doctors were able to purchase equipment to begin their own x-ray investigations. Physicists and doctors asked very different kinds of questions of these new rays, and the medical and physical investigations were largely separate. There were, however, a number of points of contact between these two worlds. Physicists were members of the first x-ray societies alongside doctors, and, by the 1920s, a small number of physicists were employed in the United States and Britain in hospitals and medical schools. In this study, I trace the contributions of these few physicists in medicine in order characterise the professional relationships that developed between doctors and physicists, individuals from very different disciplinary cultures. I show that the physicists' values of objectivity and precise measurement, along with their deep belief in regularity, often clashed with the culture of individualism in early 20th century medicine. The first doctors to use x-rays expressed faith in their own, unique clinical art, and emphasized bodies' idiosyncratic responses to radiation. These conflicting attitudes were evident in debates surrounding the best way to measure therapeutic x-rays, and I argue that the eventual adoption of the röntgen as the international unit of x-ray measurement in 1928 represents a victory of the values of physics. I link the increasing authority awarded to physicists in the American and British x-ray communities to the particular leadership roles taken on by these individuals as physics teachers and safety inspectors.

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Finally, I would like to thank my family: my husband Andrew, whose encouragement and faith in me have never wavered, my two beautiful thesis babies, Sophie and Hugh, my sister Liz for her humour and unfailing love, and my parents, Sharon and Rich, from whom I learned always to be curious, and who expressed only mild dismay when I told them I was going to be a historian and not a physicist.

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Prologue

On Friday, November 8, 1895 Wilhelm Röntgen, a physics professor at the University of Würzburg, was experimenting with a covered cathode ray tube when he unexpectedly observed crystals fluorescing some distance away. [1] A series of investigations convinced him that he was observing a new phenomenon emanating from the cathode tube. Not knowing what he had discovered, he named this new disturbance in the aether "x-rays." Twenty-five years later, Röntgen would be remembered as the "patron saint" of radiology, a medical specialty that owed its existence to this lucky observation.[2]

Röntgen was not himself interested in exploring the potential medical applications of his discovery. His first startling photograph of the bones of the human hand was just one piece of evidence amongst many in his careful investigations into the nature of these new rays. In the weeks of experimentation following his first observation, Röntgen tested the transparency of different substances to x-rays, checking the ability of the rays to penetrate wood, aluminum, rubber and even packs of playing cards. He investigated the effect that the thickness and density of a material had on its ability to block the rays. He found that he was able to observe x-ray reflection but not refraction and tried but failed to deflect the x-rays in a strong magnetic field. He published the results of his preliminary investigations at

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[1] Otto Glasser, *Wilhelm Conrad Rontgen and the Early History of the Roentgen Rays* (Baltimore: Charles C Thomas, 1934), 3. Glasser compiled an exhaustive list of English-language books and papers on x-rays published in 1896.

[2] "The Twenty-Fifth Anniversary Dinner," *Journal of the Röntgen Society* 19(1923): 100.

the end of December, only a few weeks after his initial observation.[3] This paper was translated and reprinted in *Nature* in late January and in *Science* in early February of 1896.[4]

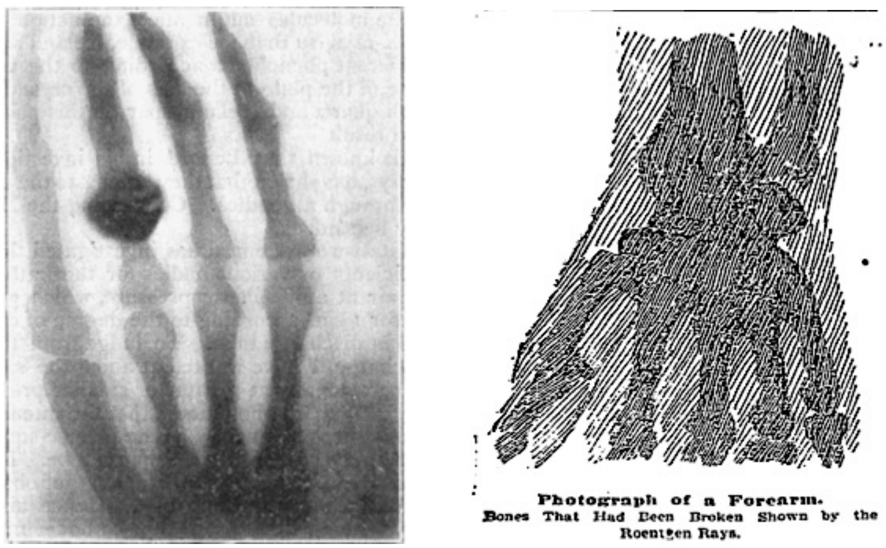

Figure 1: X-Ray of a human hand reprinted in *Nature* with the translation of Röntgen's paper and a drawing of an X-Ray picture from *The New York Times* (Feb. 18, 1896).

There were no images printed with Röntgen's first paper but he noted that he had successfully taken x-ray pictures of a compass, a piece of metal, a door, and a hand. The ability of the rays to produce a picture of the bones of the hand was offered without fanfare: "If the hand be held before the fluorescent screen, the shadow shows the bone

3 Wilhelm Conrad Röntgen, "Eine neue Art von Strahlen," *Sitzungsberichten der Würzburger Physik-medico Gegellschraft* (1895).

4 Wilhelm Röntgen, "On A New Kind of Rays," *Nature* 53(1896): 274-76.; ———, "On A New Kind of Rays," *Science* 14(1896): 227-31.

darkly, with only faint outlines of the surrounding tissues."[5] This understated observation might not have generated much interest by itself, but the ghostly pictures of hands that soon began to circulate in science journals, medical publications and in popular newspapers caused a sensation [Fig. 1].

Röntgen sent copies of his x-ray images to colleagues who were particularly taken by his picture of a human hand. O. Lummer in Berlin said that reading Röntgen's paper was like "reading a fairy tale." Lummer was especially captivated by the fact that you could "print the bones of the living hand upon the photographic plate as if by magic." R. Neuhauss similarly found the hand to be "the most curious picture."[6] The diagnostic potential of these skeletal pictures was immediately evident. Within months, doctors were sending patients with fractures, unlocatable bullets and needles embedded in various parts of their bodies to nearby physics laboratories to obtain a Röntgen picture. Within the year, these same doctors could buy their own x-ray apparatus for use in their clinics and hospitals.

In this way, Röntgen's discovery immediately moved beyond the world of physics, but the genesis of the new technology was never forgotten. Despite the modest name given to the new rays by Röntgen himself, doctors and newspapers often referred to this new phenomenon as "röntgen rays." In America, the first doctors to experiment with the new rays were "roentgenologists," their specialty "roentgenology," and the first x-ray society in Britain, The Röntgen Society, bore the name of the physicist who was seen as the founding

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[5] ———, "On A New Kind of Rays," 274.

[6] Quoted in Glasser, *Wilhelm Conrad Rontgen and the Early History of the Roentgen Rays*: 29.

father of x-ray research. When Wilhelm Röntgen died, he was memoralised in long obituaries in radiological publications.[7]

Conjured daily by his name, this German physicist travelled with each medical x-ray unit, helping to solidify the impression that this new technology was an immigrant from physics, a foreign import into the world of medicine. In 1898, Daniel Gilman, the president of Johns Hopkins University heaped praise on the many new instruments available to the physician, including the compound microscope, the stethoscope, the thermometer and the newly invented x-ray machine, boasting:

> All these captives medicine has taken from the domain of physics, and they are captives which will never be released from the service they have entered.[8]

The language chosen to describe the origins of x-ray technology did not always invoke images of coercion, but the fact that x-rays rightfully belonged to physics was never forgotten. In a 1905 catalogue of x-ray equipment, x-rays were not taken from physics but freely offered, they were in fact, "the most beautiful gift which physics has ever made to the science of medicine."[9]

The first x-ray doctors learned to read the shadowy pictures produced by the rays, to measure their intensity, and to focus them on particular parts of the body to heal skin conditions and shrink tumors. These doctors used x-rays every day. Yet with the sense that

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[7] See, for instance G.W.C Kaye, "Prof. W. K. Von Röntgen," *The Journal of the Röntgen Society* 19(1923): 47-48.

[8] Daniel Coil Gilman, *University Problems in the United States* (New York: The Century Co. , 1898), 223.

[9] Kny-Scheerer Company, *Roentgen X-Ray Apparatus and Accessories* (New York: Kny-Scheerer Company 1905), 10.

their equipment was borrowed or perhaps even stolen from physics, the early radiologists were primed to expect that they would never have full command over the instruments that defined their practice. The final expertise over röntgen-rays rested with their colleagues in physics.

1. Physics and Medicine

Wedded Bliss?—The Disunity of Science—Medical Science and "Scientific" Technology—The History of Radiology—Physicists in Medicine—A Note on Sources

1. 1 Wedded Bliss?

With the discovery of x-rays, doctors and physicists shared a common object of interest but struggled to define the boundaries of their respective expertise over this new phenomenon. Thirty years after Röntgen's discovery, in Britain, the x-ray community was fractured into two camps: the newly formed British Institute of Radiology (BIR) and the much older Röntgen Society (founded in 1897). The BIR was wholly medical while the Röntgen Society accepted both medical and non-medical members and maintained a heavy interest in the physical properties of the rays. When these two organisations amalgamated in 1927, it was hailed as "a great achievement," a triumphant reunion of the many constituents of the fractured British x-ray community under one roof.[10] In his toast to the 150 members assembled at the inaugural dinner, Sir Frank Dyson (the Astronomer Royal) attributed years of progress in radiology in Britain to the Röntgen Society, rather than the purely medical radiological societies: "The combination of medical men, physicists, and instrument makers in the one society has been of very great advantage." This new institute

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[10] Humphry Rolleston, "Address at the Amalgamation of the Röntgen Society and the British Institute of Radiology," *The British Journal of Radiology* 1 (n.s.)(1928): 5.

brought the doctors who had splintered off into separate medical societies back into contact with their colleagues in science. For Dyson,

> It was all to the good that there should be as much co-operation as possible in the furtherance of a scientific subject. Science should not be too finely divided up. … Radiology was one and indivisible, and there was the greatest good in the contact of those interested in the one branch with those interested in the other. He hoped that the two societies would have a very long and happy life together.[11]

With orange blossoms hung above his head in celebration of the "wedding" of these two societies, the new President, Sir Humphry Rolleston, (a doctor), assured his audience that "this union was no Darby and Joan affair.[12] The parties were not going to sit on each side of the fireplace. They were going to do more work in the field." Deftly avoiding questions of authority and the appropriate roles of each party within this marriage, Rolleston refused to speculate on who, "might be the bride or bridegroom," saying only that, "there was no doubt about it that the President of the Röntgen Society was the 'best man'."[13]

But there were signs that this was not a wholly peaceful or happy union. In his presidential address to the BIR, just three years after the 'wedding' celebration, Major C.E.S. Phillips's characterized the relationship between physicists and doctors as one of conquest

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[11] "Rontgen Society and British Institute of Radiology Inaugural Meeting," *The British Journal of Radiology* 1 (n.s.)(1928): 15.

[12] "Darby and Joan" is a British idiom referring to a happily married couple, enjoying a peaceful retirement.

[13] "Rontgen Society and British Institute of Radiology Inaugural Meeting," 15.

rather than partnership. Speaking to an audience representing both camps, his imagery was vivid: "In so far as the physicist has invaded territories hitherto thought to be beyond his province, he may be said to resemble the malignant forms of disease which he is so anxious to assist in eradicating."[14] Phillips was himself one of these invading physicists, and in this particular choice of metaphor he was acknowledging resistance from the medical community to the role that he and his colleagues had come to play in radiology. By the early 1930s, physicists were employed in hospitals, research institutes and medical schools, and served alongside doctors on committees charged with establishing and policing radiation safety standards. In speaking of an invasion, Phillips wasn't simply pointing out the prevalent lack of enthusiasm for introductory physics courses in medical school; he was articulating an awareness that the physicists had had to do work to establish a place for themselves in the medical world. They had not always been invited, their role was not always clear, yet their colonization of medicine, or at least some part of radiology, had been successful.

In what follows, I will examine the multiple roles played by physicists in radiology in both Britain and North America in the first three decades of the twentieth century, mapping the institutional place of physicists in radiological societies, hospitals and medical schools (Chapter 2), and then tracing the contributions of particular physicists in x-ray therapy (Chapters 3 and 4) and in the setting of safety standards (Chapter 5). Attitudes towards physics were reflected in the shape of formal institutions, in the rhetoric of both doctors and physicists, in statements of policy and in the details of actual medical practice.

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[14] C.E.S Phillips, "Presidential Address," *The British Journal of Radiology* 4(1931): 9.

Over time, as physicists took on clear roles in the radiological community, as teachers and as safety inspectors, there were accompanying shifts in radiological practice that demonstrate an increasing deference to the authority of physics.

The relationship between physicists and doctors was only one of several contested relationships whose boundaries shifted as the specialty of radiology developed and professionalized. Doctors specializing in radiology defined their epistemic and social responsibilities against those of surgeons, general practitioners, and later, radiographers, while responding to the changing expectations of their patients. Focusing on this one particular professional relationship illuminates one strand of this web while providing an opportunity to trace the dynamics of collaboration between experts from very different disciplinary cultures.

The imagery of invasion in Phillips' speech is striking precisely because it contradicts the kinds of stories usually told about the nature of this collaboration. In their history of the development of American radiology, Ruth and Edward Brecher talk of the "great debt" owed to the radiation physicists for their work establishing standards of dosimetry, providing improvements to equipment and technique, and, most importantly, their "essential" role in developing safety regulations.[15] Elsewhere in the book, the authors marvel "that the early x-ray workers accomplished as much as they did, despite their ignorance of the basic nature of the rays."[16] For Brecher and Brecher it is obvious that

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[15] Ruth and Edward Brecher, *The Rays: A History of Radiology in the United States and Canada* (Baltimore: The Williams and Wilkins Company, 1969), 451.

[16] Ibid., 31.

advancements in radiology required an understanding of the physics of x-rays, and that the knowledge that physicists had of these rays was necessary and useful to medicine.

If physics had indeed been universally viewed as foundational to radiology, then there would seem to be no reason for physicists to have to invade at all. Clear epistemic expectations of this kind would have paved the way for physicists to take on particular social roles as experts and teachers. But, as we will see, the attitudes of the early x-ray workers towards the foundational nature of physics were mixed. In 1917, for instance, the Irish physicist Edith Stoney clearly stated that, in her opinion, it was not necessary for practicing doctors to know much physics. After all, “It is easy to work with x-rays when someone else has installed them …” She recalled helping to set up X-ray departments on two British hospital ships during the first world war: “The doctors and mechanics on board had not just the needed physics, but could work the apparatus perfectly well when it was installed.” [17] According to Stoney, knowledge of the physics of x-rays played a limited role, necessary only in setting up the equipment and not afterwards.

But only a few years later we find a prominent declaration that physics was indeed foundational to the practice of radiology. An article announcing the results of the first examinations for the Cambridge Diploma in Radiology in 1920 declared that, “physics and electrotechnology are as necessary for radiology and electrology as is anatomy for surgery.”[18] To obtain this diploma, British doctors hoping to specialize in radiology had to

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[17] Edith Stoney quoted in Barbara McLaren, *Women of the War* (London: Hodder and Stoughton, 1917), 46.

[18] "The Diploma in Medical Radiology and Electrology," *Archives of Radiology and Electrotherapy* 25(1920): 164.

pass two examinations, one of which tested their knowledge of the physics of x-rays and radium. As physics became a formal component of the special training of a radiologist in both the United States and Britain in the early 1920s, expectations concerning the foundational nature of physics to this medical practice became institutionalized (Chapter 2).

Phillips' striking image of invading physicists illuminates the multiple divergent attitudes expressed by doctors, physicists and technicians towards the necessity of physics knowledge at various stages in the manipulation of medical x-rays. But, in addition, his metaphor challenges rhetoric from both historians and the actors themselves that has portrayed the collaboration between physicists and doctors as one of cooperation. When Sir William Bragg opened up the discussion of the standardization of x-ray measurement at the first International Congress of Radiology in 1925, he did so with a hope that the question would be approached by all parties, medical and non-medical, in a "spirit of compromise."[19] Even Phillips himself, in the same speech in which he spoke of invasion, assured doctors that he and his colleagues "realize that it is only by mutual understanding and cooperation that our joint labours can become fruitful."[20] Of course, those involved acknowledged their very different priorities and training. Lauriston Taylor, one of the first medical physicists in the United States, has written that he could not think of two more "incongruent bedfellows" than physics and medicine and speaks of the "laborious blending"

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[19] Sir William Bragg, "Discussion on International Units and Standards for X-Ray Work," *The Journal of the Rontgen Society* 23(1927): 65.

[20] Phillips, "Presidential Address," 16.

that was needed for productive work to be done.[21] The blending may have been difficult, but Taylor's choice of words still implies compromise on both sides.

We have, then, two very different images of the nature of the collaboration between physicists and doctors: on the one hand, there was an invasion of physicists into medicine, resulting in a forced imposition of the goals and the values of physics, and on the other, a marriage of friendly cooperation and mutual compromise between two equal parties. Of course, neither extreme adequately captures the multiple complex interactions that took place over the course of these three decades, but in general, I will argue that by the 1920s, physicists in both the United States and Britain increasingly took on leadership roles in the radiological community as safety inspectors and champions of new methods of measurement and dosimetry in x-ray therapy. If we look closely at particular moments of collaboration, we find that despite the rhetoric of cooperation, the physicists had to do very little compromising. Their values and goals dominated, despite continued resistance from practicing radiologists.

1.2 The Disunity of Science

In addition to characterizing the changing dynamics of collaboration, one aim of this study is to examine and expand upon the intuition that physics and medicine were especially "incongruent bedfellows." What in particular made these two endeavours so

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[21] Lauriston Taylor, "X-Ray Measurements and Protection, 1913-1964," ed. National Bureau of Standards (Washington: US Government Printing Office, 1981), vi.

different? Was it their objects of study? Their methods? The types of questions asked? By the turn of the century, the rhetoric if not yet the practices of a newly "scientific" medicine based on the laboratory sciences had become commonplace in the medical discourses of both the United States and Britain. The natural sciences, including physics, were quickly becoming part of the standard undergraduate medical curriculum.[22] With training in basic science and a new epistemic commitment to knowledge produced in the laboratory, we might expect doctors to share at least some common understanding of the goals and methods of science with their colleagues in physics. In Dyson's toast to the BIR, we have already seen one clear call for unity (albeit from an outsider): claiming, as he did, that radiology is "one and indivisible."

There are two separate strands of investigation that must be followed. The first is to ask whether and when doctors and physicists talked about their projects as being the same, and if so on what grounds. Were they highlighting methodological unity? Or was a reduction thought to be possible on ontological or nomological grounds?[23] The second question is whether and how this rhetoric influenced theory and action.

Scholars have shown that the very idea of unity in the sciences is itself historically situated. In his introduction to the edited collection, *The Disunity of Science*, Peter Galison traces modern talk of unity to the German scientific community of the mid-19th century, while emphasizing the influence of the particularly strong calls for unity that came from the

[22] Medical education is examined more closely in Chapter 2.

[23] For a more complex taxonomy of different kinds of 'unity' see Ian Hacking, "The Disuinities of the Sciences," in *The Disuinity of Science: Boundaries, Contexts and Power*, ed. Peter Galison and David J. Stump (Stanford: Stanford University Press, 1996), 37-74.

Vienna circle in the interwar period.[24] Ian Hacking agrees, pointing to Whewell's *The History of the Inductive Sciences* (1837) and Comte's *Cours de philosophie positive* (1830-1842) as examples of earlier 19th century works that assumed the plurality of the sciences.[25] Whether or not Galison and Hacking have definitively pinpointed the genesis of this kind of rhetoric, they nonetheless provide compelling arguments for the fundamental historicity of the idea of a unified science.

In addition, recent work in the history and sociology of science, has demonstrated the deep discontinuities in the theories, practices and epistemic values of scientists, both over long periods of time and within disciplinary communities. This body of scholarship primes us to expect that even with a new commitment to lab-based medicine, the science practiced by doctors in the early 20th century might still have been very different from the science of physicists in the same time period.

The strong sense that the history of science is dotted with insurmountable discontinuities can be traced to Thomas Kuhn's 1962 model of incommensurable paradigms. Regardless of Kuhn's intentions, *The Structure of Scientific Revolutions* has come to represent a sharp break with the vision of science offered by the logical positivists who saw scientific knowledge accumulating continuously thanks to a unified scientific

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[24] This is exemplified in the *Encyclopedia of Unified Science* edited by Rudolf Carnap, Otto Neurath and Charles Morris. Peter Galison, "Introduction: The Context of Disuinty," in *The Disuinity of Science: Boundaries, Contexts, and Power*, ed. Peter Galison and David J Stump (Stanford: Stanford University Press, 1996), 1-33.

[25] Hacking, "The Disuinities of the Sciences," 37-74.

method.[26] Kuhn has been credited by those who followed him with opening up space for social and cultural explanations of scientific change. The numerous canonical studies of scientific controversy that have followed in the constructivist tradition have not only fulfilled that goal, they have demonstrated the impossibility of identifying one, successful, transhistorical scientific method.[27] One of the most significant recent additions to this body of work is Lorraine Daston and Peter Galison's *Objectivity* (2007). In it, the authors map major historical shifts in the epistemology of observation and representation, arguing that the ideal of objectivity cannot be found throughout the Western tradition of natural philosophy; it is, instead, a 19th century invention. 'Mechanical objectivity', requiring a suppression of the will of the scientist, joined, and in many cases replaced, an older epistemic virtue that the authors label 'truth-to-nature,' which celebrated the genius of individual observers. Truth-to-nature expected idealizations where mechanical objectivity required unmediated and faithful representations of particularity and difference. Mechanical objectivity was in turn joined by a third epistemic virtue in the early 20th century, 'trained judgment,' which recovered some of the values of truth-to-nature, recognizing the need for interpretation and intuition in the scientific gaze. Instead of the sharp breaks and conversions required by Kuhn's paradigm shifts, Daston and Galison invoke the metaphor of an avalanche to describe broad changes in epistemic virtues. Individuals and communities, they argue, changed one at a time and in an unpredictable

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26 Thomas S. Kuhn, *The structure of scientific revolutions*, 3rd ed. (Chicago, IL: University of Chicago Press, 1996).

27 See for instance Steven Shapin, Thomas Hobbes, and Simon Schaffer, *Leviathan and the air-pump : Hobbes, Boyle, and the experimental life* (Princeton, N.J.: Princeton University Press, 1985).

manner, with more and more converts to the new way of knowing gathering over time.[28] Like Kuhn, Daston and Galison do not provide a causal account of these changes, but unlike Kuhn's paradigms, their epistemic virtues are not impenetrable: older ideals respond and reconfigure in the face of new ideals.

The epistemic categories identified by Daston and Galison can be used as one way of characterizing the different commitments of physicists and doctors. Moving beyond representation and into action, I will show that physicists involved in radiology in this period were more firmly committed to an ideal of mechanical objectivity, but that this commitment was not resistant to change in light of the particular challenges of using x-rays in a medical setting. The physicists did, however, become increasingly insistent and increasingly successful in arguing that an adherence to the values of mechanical objectivity was a necessarily prerequisite for progress in radiology.

But when we speak of disunity, we are not simply acknowledging these large scale historical changes in scientific practice. Competing methods and values in science can co-exist across different disciplines and even within the same discipline at one time. In her ethnographic study of laboratories in high energy physics and molecular biology, Karin Knorr Cetina introduces the idea of 'epistemic cultures' to capture the very different ways in which scientific knowledge is produced in different disciplines.[29] She argues that

> science and expert systems are obvious candidates for cultural division; they are

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[28] Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007), 49.

[29] K. Knorr-Cetina, *Epistemic cultures: how the sciences make knowledge* (Cambridge, Mass.: Harvard University Press, 1999). See also Karin Knorr-Cetina, "The Care of the Self and Blind Variation: The Disuinity of Two Leading Sciences," in *The Disunity of Science*, ed. Peter Galison and David J. Stump (Stanford: Stanford University Press, 1996), 286-310.

> pursued by groupings of specialists who are separated from other experts by institutional boundaries deeply entrenched in all levels of education, in most research organizations, in career choices, in our general systems of classification.[30]

The differences in the "machinery of knowledge production" that Knorr-Cetina outlines are stark: where molecular biology is highly individualistic and experiential, high energy physics is communal, manipulating layers of signs rather than direct empirical data. These late 20th century communities are worlds apart, their inhabitants encultured by specialized education, professional societies and entrenched traditions of social organization.

But if we magnify our gaze to look at the fine detail of any one community, we find that each discipline is itself far from homogenous. Peter Galison, for instance, has identified three subcultures within 20th century physics: experimentation, instrumentation and theory, each with independent historical trajectories. Breaks in experimental practice, he argues, do not necessarily coincide with major shifts in theory and instrumentation. And in turn, each of these subcultures of physics has its own fine structure of competing communities. Galison identifies and follows two prominent instrument traditions in mid-20th century nuclear and particle physics that he labels 'image' and 'logic', each with its own set of practices, skills and styles of argument and evidence.[31]

In the story that follows, I will pay attention to this kind of fine structure within disciplinary communities. There is no moment at which a single, homogenous community of doctors or physicists speaks with a unified voice. I will highlight national differences in

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[30] Knorr-Cetina, *Epistemic cultures*: 2.

[31] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

attitudes towards the role of physics and towards the proper methods and goals of radiology, as well as differences between physicists and between doctors from the same national community. The broad claims that I *do* make about the particular goals, values and epistemic commitments of physicists versus doctors will continually be complicated by the disunity of both of these communities.

1.3 Medical Science and "Scientific" Technology

Historians of medicine have tended to assume that the new medical technologies of the late 19th and early 20th centuries—sphygmomanometers, electrocardiographs, x-ray machines— signified the newly scientific nature of medical practice. Joel Howell has called the x-ray machine in particular "the symbol of advanced scientific medicine,"[32] and John Harley Warner has argued more generally that these increasingly ubiquitous medical machines were "gleaming emblems of science."[33] In his analysis of the x-ray, Stanley Joel Reiser implicitly assumes that it was the x-ray's association with physics that granted the new technology a solid epistemic footing in medicine:

> At the time the x-ray was invented, it not only carried the prevailing ideology about the photograph's nature [as objective reality]. It was also born in a scientific laboratory, described by a prominent investigator, and revealed in a journal of

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[32] Joel D. Howell, *Technology in the hospital: transforming patient care in the early twentieth century* (Baltimore: Johns Hopkins University Press, 1995), 104.

[33] John Harley Warner, "The History of Science and the Sciences of Medicine " *Osiris* 10(1995): 104.

> physics. With such a heritage, the x-ray entered medicine with significant standing as an objective depiction of illness.[34]

But, given the disunity of the sciences, this demonstrated association with science does not really tell us much about the meanings embedded in these machines for various users.

The meaning of "scientific medicine," on the other hand, *has* received close scrutiny from historians who have documented the changing and conflicting attitudes towards the role of basic science in medicine and status of medicine as a science itself. Late 19th and early 20th century American medical reformers who successfully drew on the rhetoric of science to promote a German model of lab-based medicine, criticized earlier practice for being 'unscientific.' But Warner has shown that the previous generation of American doctors had similarly seen themselves as scientific reformers, but with an epistemology based on a clinical empiricism inspired by the Paris school.[35] Medical practice informed by basic sciences like physiology and bacteriology has been only one possible vision of scientific medicine. And as a series of seminal studies from the late 1970s demonstrated, there was in fact widespread skepticism towards the German model in the early twentieth century.[36] Recent work has shown, in addition, that the rhetoric of scientific medicine was

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[34] Stanley Joel Reiser, *Technological Medicine: The Changing World of Doctors and Patients* (Cambridge: Cambridge University Press, 2009), 27.

[35] John Harley Warner, "The Fall and Rise of Professional Mystery: Epistemology, Authority and the Emergence of Laboratory Medicine in Nineteenth Century America," in *The Laboratory Revolution in Medicine*, ed. Andrew Cunninham and Perry Williams (Cambridge: Cambridge University Press, 1992), 110-41.; ———, "Ideals of Science and their discontents in Late Nineteenth-Century American Medicine," *Isis* 82(1991): 454-78.; ———, *The Therapeutic Perspective: Medical Practice, Knowledge and Identity in America* (Cambridge: Harvard University Press, 1986).

[36] Gerald Geison, "Divided We Stand: Physiologists and Clinicians in the American Context," in *The Therapeutic Revolution: Essays in the Social History of American Medicine*, ed. Morris Vogel and

not confined to regular or allopathic physicians; 19th century homeopathic physicians saw their practices as scientific as well.[37] There is broad historical consensus that the very category of 'scientific medicine' has always been contested and contestable, yet the role played by medical technology in shaping or reinforcing these meanings had received very little scrutiny.

Stanley Joel Reiser's sweeping *Medicine and the Reign of Technology* (1978) documents the arrival of numerous new diagnostic devices including the microscope, stethoscope and x-rays, but does not address the question of the relationship of these devices to any of the rhetorics of scientific medicine. Employing an unspoken technological determinism, Reiser emphasizes (and criticizes) the increasingly distant relationship between doctor and patient caused by these new devices.[38] Christopher Lawrence has presented one of the more sophisticated analyses of what exactly it might mean for an instrument to be "scientific" in his study of divergent attitudes towards the sphygmomanometer in the British medical community. He argues that for some doctors this device represented a threat to the clinical art by promising to render the process of

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Charles Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), 67-90.; Russel C. Maulitz, "Physician vs Bacteriologist: The Ideology of Science in Clinical Medicine " in *The Therapeutic Revolution: Essays in the Social History of American Medicine* ed. Morris Vogel and Charles Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), 91-107.; Charles Rosenberg, "The Therapeutic Revolution: Medicine, Meaning and Social Change in 19th Century America," in *The Therapeutic Revolution: Essays in the Social History of American Medicine*, ed. Morris Vogel and Charles Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), 3-26.

[37] Mark Weatherall, "Making Medicine Scientific: Empricism, Rationality and Quackery in Mid-Victorian Britain," *Social History of Medicine* 9(1996): 175-94.

[38] Stanley Joel Reiser, *Medicine and the reign of technology* (New York: Cambridge University Press, 1978).

diagnosis wholly mechanical. For these physicians the diagnostic art, nurtured by years of individual experience, could not be codified or explained. Alternately, those who advocated a larger role for science in medicine celebrated the scientific way of seeing and reasoning encouraged by these kinds of diagnostic tools.[39] Lawrence speculates further that for these champions of scientific medicine, the mere presence in the clinic of these physiological instruments, as "children of the experimental sciences," represented the promise of the utility of those basic sciences to medicine.[40]

Lawrence's analysis is important for a number of reasons. He draws attention to the multiple meanings of "medical science" and he highlights divergent attitudes towards the place of science in medicine. But Lawrence's study still does not shake the initial claim that new medical technologies were universally identified as 'scientific' by all witnesses. By documenting divergent attitudes towards the sphygmomanometer, he complicates the question of audience inherent in any argument about the symbolism of a technology. For whom does a particular technology signify science? For doctors (all doctors?) or patients or both? And what does "science" mean in each case?

These questions are made all the more compelling by dominant arguments claiming that it was the rhetoric rather than the actual products of scientific medicine that established physician's status in the late 19th century. S.E.D Shortt has argued that "medicine gained prestige not through enhanced therapeutic efficacy, but as a result of an

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[39] Christopher Lawrence, "Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain, 1850-1914," *Journal of Contemporary History* 20(1985): 503-20.

[40] Lawrence, 517.

increasing public faith in science."[41] Arguing along similar lines, Geison has stated that, like Latin, experimental techniques gave medicine, "a new and culturally compelling basis for consolidating its status as an autonomous 'learned profession.'"[42]

X-ray technology was certainly part of this rhetoric. Manufacturers often capitalized on this in their advertising to doctors, and later, to dentists. Advertising in the early 1930s, the Ritter Dental Manufacturing company assured dentists that the "Ritter X-Ray Machine conveys the correct psychological effect on the patient, creating an impression of scientific assurance… "[43] Ritter's advertising explicitly acknowledged the link between the presence of these machines and patient trust: "Lacking scientific knowledge, the patients must largely base their impressions of a practitioner's skill on the environment of the dental office, and on the nature of his equipment."[44]

In actual practice, the early American and British radiologists *were* largely committed to a scientific medicine, but theirs was often a vision of science that did not conform to the epistemic commitments and goals of their colleagues in physics. In addition, there remained a significant constituency of radiologists utilizing these advanced tools of science who, like the doctors in Lawrence's study, continued to emphasize the holistic

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[41] S.E.D Shortt, "Physicians, Science and Status: Issues in the Professionalization of Anglo-American Medicine in the 19th Century," *Medical History* 27(1983): 67.

[42] Geison, "Divided We Stand: Physiologists and Clinicians in the American Context," 85.

[43] Ritter Dental Ritter Dental Company, *Better Radiography with the Ritter Dental X-Ray Machine* (New York1933), 3.

[44] Ritter Dental ibid., 36.

nature of medicine, arguing that medicine was fundamentally an art, not a science.[45] That these doctors were able simultaneously to make this argument and utilize their x-ray technology shows how carefully we must attend to divergent meanings attached to particular technologies.

1.4 The History of Radiology

This study focuses on the English speaking communities of radiologists in the United States and Britain (Canadian radiologists participated in American societies until they formed their own separate body in 1937). Despite important differences in the place of physics in American and British medical education, in the methods of measuring x-rays preferred in each country, and even in the initial enthusiasm with which physicists were greeted in x-ray societies, the broad contours of my argument remain the same in each national context. By the 1920s, doctors in both countries increasingly deferred to the authority of the few physicists in medicine. This deference could certainly be documented in either country on its own, but where a comparative study becomes helpful is in the development of explanations for that deference. Along with the many differences in the structure and practice of radiology in each community, it is possible to identify key similarities, and it is those similarities that I have taken to be crucial. By the 1920s, physicists began to occupy similarly prominent roles in the x-ray communities in both countries, as teachers of physics in post-graduate radiology programs and as leaders in the development of safety guidelines

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[45] See especially Chapter 4 and Christopher Lawrence and George Weisz, eds., *Greater than the Parts: holism in biomedicine, 1920-1950* (Oxford: Oxford University Press, 1998)..

for medical x-rays. I argue that it was through these particular social roles that the authority of physicists was nurtured and maintained.

The early radiological communities in the United States and Britain have each received attention in a standard history. For the United States this is given in Ruth and Edward Brecher's *The Rays: A History of Radiology in the United States and Canada* (1969), and for Britain, in E. H. Burrow's' *Pioneers and Early Years: A History of British Radiology* (1986).[46] Both provide detailed accounts of the first societies, journals, personalities and technical innovations. Similarly straightforward accounts of the development of radiology in particular local contexts also exist.[47] Bettyan Holtzman Kevles' *Naked to the Bone: Medical Imaging in the Twentieth Century* (1997) moves beyond a strictly internalist narrative to consider the influence of major socio-political events, including the first world war, on the development of medical imaging in the United States. In addition, she uses evidence from the legal record as a test of the wider acceptance of evidence from various types of imaging modalities, documenting changes to the technology alongside cultural responses to these new ways of seeing.

Several scholars have investigated the reaction to X-rays in Western culture. A rich record of poems, editorials and cartoons reveals a conflicted fascination with the new rays.

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[46] See also Jean Margaret Guy, "The development of Radiology in Britain 1896-1921 and factors influencing its growth" (University of Cambridge, 1993)., unpublished MD thesis.

[47] For Ireland, see Horst Babel and J. C. Carr, *A century of medical radiation in Ireland : an anthology* ([Dublin]: Anniversary Press, 1995). For Scotland, see John F. Calder, *The history of radiology in Scotland 1896-2000* (Edinburgh: Dunedin Academic Press, 2001). For Toronto see Edward Shorter, *A century of radiology in Toronto* (Toronto: Wall & Emerson, 1995). For the development of x-ray and radium therapy in Ontario see Charles Hayter, *An element of hope: Radium and the response to cancer in Canada, 1900-1940* (Montreal: McGill-Queen's University Press, 2005).

Fashionable, urban women rushed to commercial x-ray studios to obtain radiographs of their hands, even as x-ray-proof undergarments were sold to protect female modesty from the indecent gaze of the new rays. X-ray pictures were a curiosity, revealing hidden structures of the body, but they also promised the thrill of danger—not physical danger in those early years—but an encounter with something other-worldly, deathly, even supernatural.[48] Through these stories we catch glimpses of some of the possible experiences of patients encountering this new technology for the first time. Historians have focused in particular on reactions to the startling, skeletal x-ray image. Radiographs of hands circulated in newspapers and journals, reaching a wide audience of individuals who would not necessarily have had the opportunity to witness an x-ray apparatus in action. But for the patients who did submit to x-ray examination, it was not just the x-ray image but the *creation* of that image that was itself a strange encounter with technology. The x-rays were invisible, but in a darkened room, it would have been impossible to ignore the unshielded gas tube, glowing a soft green, and the loud crackling of the purple lightning sparks generated by the high voltage electric machine. This eerie display may certainly have symbolized science to these patients, but if so, it was likely a science closer to magic.[49]

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[48] See Howell, *Technology in the hospital*., esp. Chapter 5, "The X-Ray Image: Meaning, Gender and Power," p.133-168"; Lisa Cartwright, *Screening the body: tracing medicine's visual culture* (Minneapolis: University of Minnesota Press, 1995)., esp. Chapter 5, "Decomposing the Body: X-Rays and the Cinema," p. 107-142 and Chapter 6, "Women and the Public Culture of Radiography," p. 143-169"; Allan Grove, "Rontgen's Ghosts: Photography, X-rays and the Victorian Imagination," *Literature and Medicine* 16, no. 2 (1997): 141-73.

[49] Sofie Lachapelle has demonstrated the blurring of lines between magic and science for late 19th century audiences. See Sofie Lachapelle, "Science on Stage: Amusing Physics and Scientific Wonder at the Nineteenth-Century French Theatre," *History of Science* 47(2009): 297-315.

The doctors who first used x-rays were also witnessess to these strange phenomena, and encountered these glowing tubes and sparking electric devices for the first time when they took a patient to an x-ray lab or turned on their own, newly acquired apparatus. As educated professionals, early 20th century doctors represent a special audience for popular science, an audience of individuals with close professional relationships with real scientists. Literature on popular science has emphasized both the agency of popularizers and of readers to actively reconfigure the knowledge presented. Science in popular form was not merely simplified, but mobilized and interpreted in particular ways.[50] We have yet to fully understand what influence these informal exposures to science had on those professionals, like doctors, whose practice depended on negotiated interactions with scientists and technology. In this study, I have looked for more immediate causes to explain the increasing deference of doctors to the values of physics, but I hope that I have left open the possibility that these shifts can also be understood as echoes of broader cultural changes in the perception of physics.

The first doctors to use x-rays operated within a web of professional associations, interacting with their medical colleagues, hospital administrators, technicians and nurses, as well as equipment manufacturers, insurance companies, and members of the legal system. Historians have explored the dynamics of some of these relationships, however the map remains unfinished. Tal Golan has placed the early movements towards

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[50] See for instance, Roger Cooter and Stephen Pumfrey, "Separate Spheres and Public Places: Reflections on the History of Science Popularization and Science in Popular Culture," *History of Science* 32(1994): 237-67., Stephen Hilgartner, "The Dominant View of Popularization: Conceptual Problems, Political Uses " *Social Studies of Science* 20(1990): 519-39., Bernard V. Lightman, *Victorian science in context* (Chicago, Ill.: University of Chicago Press, 1997).

professionalization in the American radiological community within the context of concern over the use of x-ray images in legal cases. Doctors pushed to ensure that the X-ray pictures admitted as visual evidence in trials were understood to require expert medical interpretation.[51] Joel Howell has situated the early use of x-rays in a narrower institutional context, arguing that the widespread use of x-rays and other diagnostic medical technologies in American hospitals wasn't possible until particular administrative procedures were in place. These included standardized forms and a systematic division of labour that was inspired in part by a movement towards the ideals of Taylorism and scientific management.[52] The shifting boundaries between radiotherapists and surgeons, and between doctors and x-ray technicians have also been explored in particular local contexts.[53] Relationships between physicists and doctors occupy only one small part of this professional web. By the end of the 1920s there were less than 20 physicists directly involved in radiology, while there were hundreds of doctors using x-rays in both the United

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[51] Tal Golan, "The Emergence of the Silent Witness: The Legal and Medical Reception of X-Rays in the USA," *Social Studies of Science* 34, no. 4 (2004): 469-99.

[52] Howell, *Technology in the hospital*.

[53] For the relationship with surgeons in Britain see Ornella Moscucci, "The "Ineffable Freemasonry of Sex": Feminist Surgeons and the Establishment of Radiotherapy in Early Twentieth-Century Britain," *Bulletin of the History of Medicine* 81(2007): 139-63.; in Canada, see Hayter, *An Element of Hope*.; in Spain, see Rosa María Medina Doménech, "Scientific Rhetoric in the Consolidation of a Therapeutic Monopoly," *Social History of Medicine* 10, no. 2 (1997): 221-42. The gendered rhetoric surrounding the development of radiography in Britain is examined in Anne Witz, "Gender and Radiography," in *Professions and Patriarchy* (London: Routledge, 1992), 162-84. The boundary between medical and non-medical x-ray workers in the United States in the early 20th century is discussed in Golan, "The Emergence of the Silent Witness," 469-99. See also Rebecca Herzig, *Suffering for Science: Reason and Sacrifice in Modern America* (New Brunswick: Rutgers University Press, 2005)., Ch. 5, “Martyrs,” p. 85-99. Herzig argues that the boundary between medical and non-medical x-ray workers was maintained in part through a professional rhetoric of willing sacrifice in the face of known danger.

States and Britain. These physicists were few in numbers, but disproportionately influential in shaping safety policies and dosing practices in radiotherapy.

1.5 Disciplines and Trading Zones

The question at the heart of this project has much in common with sociological accounts of the development of professions in general and specialization in medicine in particular. In his analysis of the dynamics between medical specialties, Andrew Abbot offers a Darwinian model, arguing that we must think of the system of professions as an ecological, interdependent system. Individual professional groups compete for control of particular jurisdictional boundaries which are always in dispute.[54] In a classic Marxist analysis, Abbot assumes that professional groups will perpetually be in conflict because at the heart of their interaction is a concern over "the control of work."[55] This model is well suited to capturing the kinds of conflicts that arose between radiotherapists and surgeons who both claimed sites of malignant tumours as their proper jurisdiction. Physicists and doctors both laid claim to x-rays, but there was broad agreement from the beginning that physicists would investigate the physical properties of these new rays while doctors would utilize their photographic qualities and investigate their action on the body. In the early period, there was some direct competition over work: a handful of physicists, for instance, were in charge of hospital x-ray departments before and during WWI, but, in general,

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[54] Andrew Delano Abbott, *The system of professions : an essay on the division of expert labor* (Chicago: University of Chicago Press, 1988).

[55] Ibid., 19.

doctors were successful in decreeing that diagnosis and the interpretation of the x-ray images were only properly done by those with medical training. Physicists were successful in creating new spaces in which to work without displacing doctors. When disagreements did arise, they did not concern who should properly occupy these new spaces, but rather whether these spaces should exist at all. A number of doctors, for instance, questioned the utility of abstract physical knowledge to medical practice. To these doctors, it was not obvious that the services of physicists were always necessary or that the problems identified by physics were even medically relevant.

But despite this skepticism from some individual doctors, physicists *were* consulted, doctors and physicists were members of the same committees charged with answering questions to do with dosage and safety standards, and physicists were invited to teach radiologists-in-training. We need a model, then, that captures the dynamics of these moments of collaboration. Historians, philosophers and sociologists of science have offered a number of analytical tools designed to express certain key features of contact between different knowledge cultures, including trading zones, boundary objects and trafficking materials. The model that proves to be the most flexible is a modified version of Peter Galison's trading zones.

Galison's idea of a trading zone is widely used by scholars studying the process by which different disciplines learn to communicate and coordinate action.[56] He developed this metaphor to capture the interaction of the subcultures of physics – experiment, theory

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[56] Galison, *Image and Logic*. See especially Chapter 9, "The Trading Zone: Coordinating Action and Belief," p. 781 – 844.

and instrumentation. Defining cultures as groups with different symbolic systems, Galison uses standard sociological criteria including journals, meetings, and informal avenues of exchange to distinguish his three communities of 20th century physics. Prior to the discovery of x-rays, doctors and physicists certainly met these kinds of criteria for separate communities. With the exception of a number of medical schools that were beginning to introduce physics into the undergraduate medical curriculum, there was very little contact between doctors and physicists and certainly no professional contact outside of these educational encounters. After the discovery of x-rays, doctors and physicists joined the same newly established x-ray societies and published in the same x-ray journals. These initially appear to be perfect candidates for trading zones, but it turns out that these encounters failed to meet crucial criteria.

Galison defines a trading zone as a site (not necessarily spatial), in which cultures can coordinate action, such as the exchange of knowledge, without agreeing on a global meaning of objects and terms exchanged. This model has obvious appeal—if doctors and physicists meet in a trading zone, they don't have to agree on any aspect of the important properties of x-rays or how best to speak about them and they don't have to share a common goal in order to provide something of value to each other. Coordinated action is made possible through the development of increasingly sophisticated new languages that develop out of existing languages to meet the needs of the trading partners. At first this is simple "foreigner talk," and then a "pidgin" constructed of elements of the two native languages, and finally a sophisticated "creole" that can serve as a native language in itself. The example of a trading zone that Galison appropriates from anthropologists is the exchange of money that occurs between peasants and land owners in Columbia. Each of

these communities has a different understanding of money. For the land-owners, money is a neutral entity, where for the peasants, money acquired in a certain way takes on a special purpose. When a child is baptized, for instance, the money is secretly baptized instead and is then sent off into circulation where it will faithfully return to its family with more money in tow. The peasants and the landowners, Galison argues, are still able to trade without agreeing on meanings attached to the money exchanged. A strong example of a physical trading zone in Galison's study is the wartime Radiation Lab at MIT. The research undertaken there was coordinated by the very different cultures of theoretical physicists and engineers. A particularly important feature of this negotiation and of learning how to interact successfully in a trading zone was a mutual process of transformation. The engineers learned some physics and the physicists learned how to think like engineers. Galison goes on to trace this engineering influence into particular strands of post-war theoretical physics.

Besides mutual transformation of the trading parties, the second important feature of Galison's trading zones is epistemic parity between the subcultures. Galison argues that, "no level is privileged, no subculture is the arbiter of progress in the field or serves as a reduction basis."[57] This is linked to Galison's intercalated model of change in physics: there can be discontinuities in particular groups that may or may not affect the others. A theory can change while instruments remain constant. This seems to be both descriptive and normative for Galison; he argues that disunity is the source of stability in physics. The criterion of epistemic parity, then, lies at the heart of the trading zone. Each group may at

[57] Ibid., 799.

times attempt to impose their particular understanding on the other but in the end each remains mostly independent. If there is a process of transformation, it is mutual.

These two key features of Galison's trading zones - epistemic parity and mutual transformation – are missing from the interactions of the doctors and physicists in this study. If there was a transformation, it was largely the doctors who transformed. In part because of the increasing cultural authority of physicists, these actors did not come to the table on an equal epistemic footing.

Another model which is widely used is Susan Leigh Star and John Griesemer's "boundary object." Star and Griesemer developed this idea in their analysis of the interaction of museum managers, professional biologists and amateur naturalists in early 20th century California. Like Galison, these authors were interested in understanding how action can be coordinated without shared meaning or even shared goals, but rather than looking at language, they focus on the role of certain material objects. Objects become boundary objects when they "are plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites."[58] In Star and Griesemer's story, these objects include specimens, field notes, atlases, maps and even the state of California itself. Diverse groups interact through these objects which maintain a separate meaning and identity for each group.

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[58] Susan Leigh; Griesemer Star, John, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkely's Museum of Vertebrate Zoology, 1907-39," *Social Studies of Science* 19(1989): 391.

There is the possibility of a greater heterogeneity in this model; in their interactions with these boundary objects, the amateur naturalists did not become professional biologists. It was not necessary, for instance, for the amateurs to develop an opinion on Darwin in order to collect specimens for the museum. And where Galison's insistense on epistemic equality appears normative, theirs is purely methodological. The authors argue that the historian or sociologist shouldn't "presuppose an epistemological primacy for any one viewpoint."[59] This is clearly methodologically sound, and leaves open the possibility that the historian or sociologist will find historical actors who do impose or grant this kind of authority to another group.

In her history of the Vienna Radium Institute, Maria Rentetzi adds motion to boundary objects, calling these fluid objects "trafficking materials." Trafficking materials, she argues, have the ``ability to take on multiple identities, not because they are shared between different worlds but because they are transferred across them."[60] Individuals in her story are able to take advantage of the mobility of radium, and move with it through multiple social worlds, including medicine, physics and industry. But while she traces this movement of radium and radium experts, Rentetzi leaves us curious about the finer details of changes in meaning that must have occurred as disciplinary boundaries were crossed.

As models, both trading zones and boundary objects (or trafficking materials) stress cooperation and start from an assumption of epistemic parity between multiple meanings and identities. These models set out to understand how and why moments of

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[59] Ibid., 389.

[60] Maria Rentetzi, "Trafficking Materials and Gendered Experimental Practices: Radium Research in Early 20th Century Vienna." (Gutenberg-e Online History Series, 2007), www.gutenberg-e.org.

interdisciplinary communication or collaboration are successful. But because they start out assuming success, both models become evaluative and prescriptive.[61] In a case study looking at MRI, Davis Baird and Mark Cohen explicitly set out to evaluate the success of the trading zones surrounding this particular technology. Baird and Cohen point out that Galison fails to address the motivation of the trading partners, and argue that the motivation for trading is crucial to the success or failure of a trading zone. They argue that we must distinguish between trading that takes place within a gift economy and that which takes place within a commodity economy. In this analysis, the stability of Galison's trading zones is attributed to the fact that the subcultures of physics have a shared goal and common purpose and are therefore engaged in the community building activities of a gift economy. When commercial interests enter, as they do in the case of MRI, Baird and Davis see instability emerging, evidenced by examples of miscommunication between trading partners.[62] My goal with this study is not to evaluate the overall success or failure of various moments of collaboration. I am not sure how we could effectively establish some absolute standard of success beyond the estimation of those directly involved. Davis and Baird's own examples demonstrate that doctors and engineers often have very different standards themselves. Instead, I hope to highlight various possible strategies for collaboration, while exploring the reasons for shifting dynamics of trade.

[61] In his study of the use of chaos theory in multiple fields, Stephen Kellert sets out explicitly to develop a normative theory of interdisciplinarity. See Stephen Kellert, *Borrowed Knowledge: Chaos Theory and the Challenge of Learning Across Disciplines* (Chicago: Chicago University Press, 2008).

[62]Davis Baird and Mark Cohen, "Why Trade?," *Perspectives on Science* 7, no. 2 (1999): 231-54.

While the idea of a trading zone has been appealed to in a numerous contexts, both within HPS and in other fields,[63] the concept itself has received little sustained analysis. The only existing modification and expansion on the idea of a trading zone has been developed by Harry Collins, Robert Evans and Mike Gorman.[64] Their new model takes into account the questions of authority that occupy this study without attempting to be prescriptive or to evaluate the ultimate success and failure of various types of trading zones. As part of what Collins and Evans have elsewhere called the "Third Wave of Science Studies,"[65] a project concerned with understanding experts and expertise, they have imported some of their taxonomy of experts into Galison's trading zones. Their new, 2-dimensional model (Figure 2) introduces two axes, one of which measures the extent to which trading partners maintain heterogeneity while the other axis measures cooperation versus coercion, allowing us to describe and account for a disparity of authority and power between the trading parties. Trading zones as Galison envisioned them end up in the top left hand corner of the new map, in "Interlanguage" zones exhibiting the strongest homogeneity and cooperation. The "Fractionated" trading zones in the top right hand corner are still cooperative, but the groups involved maintain their separate identities.

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[63] See for instance Xiang Huang, "The Trading Zone: An Examination of Jesuit Science in China," *Science in Context* 18, no. 3 (2005): 393-427., Michael E Gorman, James Groves, and Jeff Shrager, "Societal Dimensions of Nanotechnology as a Trading Zone: Results from a Pilot Project," in *Discovering the Nanoscale*, ed. Davis Baird, Alfred Nordmann, and Joachim Schummer (Amsterdam: IOS Press, 2004), 63-73., Katherine C. Kellog, Wanda J Orlikowski, and Jo Anne Yates, "Life in the Trading Zone: Structuring Coordination Across Boundaries in Postbureaucratic Organization," *Organization Science* 17(2006): 22-44., David Mills and Mary Taylor Huber, "Anthropology and the Educational 'Trading Zone': Disciplinarity, pedagogy and professionalism," *Arts and Humanities in Higher Education* 4(2005): 9-32.

[64] Harry Collins, Robert Evans and Mike Gorman, "Trading Zones and Interactional Expertise," *Studies in the History and Philosophy of Science* 38 (2007): 657-66.

[65] Harry Collins and Robert Evans, "The Third Wave of Science Studies: Studies of Expertise and Experience," *Social Studies of Science* 32(2002): 235-96.

Here the trading might be mainly material, in the form of boundary objects, or primarily linguistic in which case both groups have gained interactional expertise that allows them to communicate with the other while still maintaining their own cultures. The bottom right gives the extreme case of an "Enforced" trading zone in which neither of the cultures transforms due to their interaction but meaning is imposed by one dominant culture on the other. In the "Subversive" trading zones on the bottom left, meaning is imposed by one culture on the other, resulting in a gradual transformation so that the cultures become homogenous. Here the transformation is entirely one-sided; one culture morphs into the other.

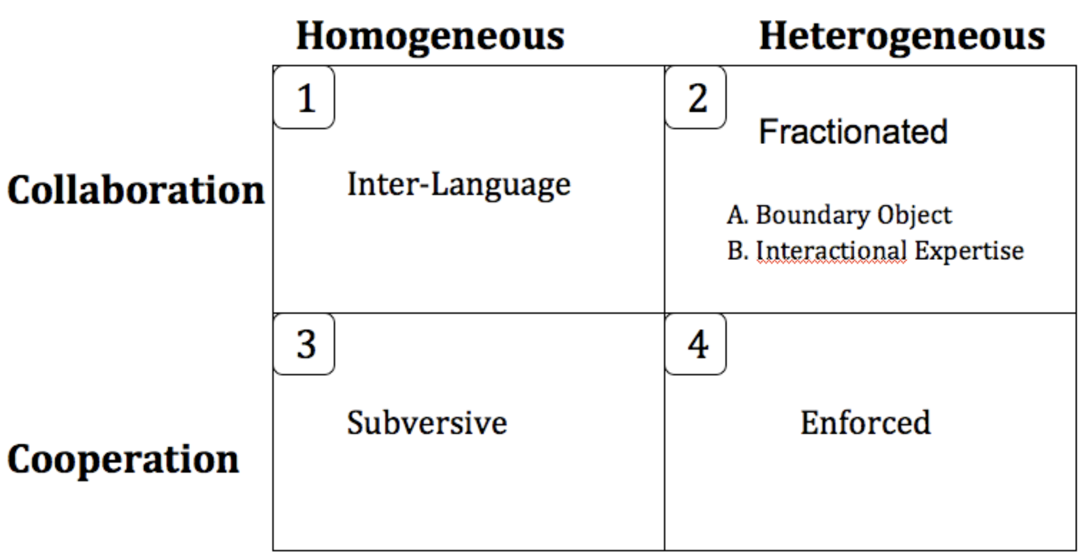

Figure 2: Two dimensional map of different trading zones. From Collins et al., 2007.

This model is helpful in tracing the dynamics of collaboration between doctors and physicists, not only to characterize these dynamics as they existed but also as they were ideally envisioned by the actors. A particular strength of this model is that it is not a static

map; trading zones can evolve and change their characteristics over time.[66] In the beginning, doctors and physicists met in fractionated trading zones, with clear heterogeneous identities, developing both boundary objects and interactional expertise. But over time, as physics became a standard part of radiology curriculum, and as the physicists' particular vision of standardization and quantification came to dominate, the interactions became more like those described in the bottom half of the map. As teachers, safety inspectors and experts on questions of dosage, physicists came to control access to a body of elite knowledge that was deemed crucial to the future success of radiology. Doctors gained some of this knowledge, but, at least in the period under study, there was no reciprocal stipulation for physicists involved in hospital work to gain formal training in medicine or biology. By the beginning of the 1930s, training in physics had become a central component of the identity of radiologists and radiotherapists in both the United States and Britain, moving the trading zones between physicists and doctors closer to type that Collins et al. label "subversive." Just as Einsteinian physics language "colonized" Newtonian physics,[67] when doctors spoke of x-rays, they increasingly thought in terms approved by the physicists.

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[66] In her study of the development and use of a device to prevent sea turtles from being caught by shrimp fishermen, Lekelia Jenkins uses this new model of trading zones. She argues that the trading zones in her case study did evolve from an enforced to a more collaborative interaction between scientists and the shrimpers. Lekelia Jenkins, "The evolution of a trading zone: a case study of the turtle excluder device," *Studies in the History and Philosophy of Science* 41(2010): 75-85.

[67] Collins, "Trading Zones and Interactional Expertise," 660.

1.6 A Note on Sources

The richest sources for this study have been the radiological journals published in both countries. For Britain, I have relied most heavily on *The Journal of the Röntgen Society*, *The Archives of the Roentgen Ray*, *Medical Electrology and Radiology, The Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* and *The British Journal of Radiology.* For the United States, I examined *The American Quarterly of Roentgenology, The American X-Ray Journal, The Transactions of the American Roentgen Ray Society, The Journal of Roentgenology, Archives of Radiology and Electrotherapy* and *Radiology.* These journals contain details of the membership and constitutions of the first x-ray societies, reports from meetings, specific plans for many hospital radiology departments, medical school curricula, as well as the papers presented at meetings, and perhaps most informatively, the conversations that often took place following the presentation of these papers. In addition to these journal sources, I made use of many early radiology textbooks written by both doctors and physicists. These texts, and the clinical and research papers in the journals have allowed me to chart changes in practice as well as differing attitudes concerning x-ray measurement and safety. I have also made extensive use of trade literature from both countries to establish the kinds of equipment available to doctors as well as the ways in which this equipment was advertised. Finally, my time spent in museums becoming familiar with actual equipment from this period has helped me to begin to envision the experience of turning on one of those first sparking, glowing x-ray machines.

In the following chapters I will introduce the first medical physicists and make an argument for the crucial role that they came to play in radiology in both the United States and Britain by the end of the 1920s.

In Chapter 2, I will outline the careers of the first physicists to take up posts in hospitals and medical schools. These physicists were not always welcome in the first x-ray societies, but they were increasingly called on to teach physics to doctors following the First World War, as post-graduate programs in radiology were developed in both countries. These programs stressed the foundational nature of physics to radiology, creating clear expectations among radiology students of the epistemic authority of physicists.

Chapters 3 and 4 tell the story of x-ray therapy, and show the impact of this increasing deference to physics on actual radiological practice. In Chapter 3, I show that divergent opinions among physicists and doctors concerning the best way to measure a dose of x-rays began to be evident as committees were formed to examine the problem of x-ray measurement. Doctors concerned with idiosyncratic bodies and unstandardised equipment did not share physicists' desire for precision, or their trust in regularity.

In Chapter 4, I show that even though practice had stabilized by the early 1920s, with doctors in both countries using quantitative measurements to establish a dose of x-rays, physicists increasingly insisted that these measurements were inadequate. These physicists pushed for their preferred unit of measure, the röntgen, and were successful in establishing this as the international unit of x-ray measurement in 1928. This unit gradually came into medical use, despite some continued strands of resistance from doctors emphasizing their clinical art.

Chapter 5 documents the leadership roles taken on by physicists in questions of safety. Physicists were the first to call attention to inadequate protective devices in the late 1910s, and were prominent members of the first safety committees. Without clear clinical or physiological data, the official safety guidelines adopted in the United States and Britain in the 1920s were primarily based on guesses as to the maximum safe dose of x-rays, coupled with physics research into the penetrability of various types of materials to the rays. These quantitative guidelines ignored doctors' continued belief in idiosyncratic responses of different bodies to x-rays, and so, like the eventual adoption of the röntgen, demonstrate the increasing influence of physics in radiology. But the story of safety offers additional insight into how that authority was maintained. Physicists were called on to police the new standards, fulfilling a reassuring social function that reinforced their special expertise.

The defined roles of physicists, technicians and doctors involved in x-ray work were in flux for much of the early decades of the twentieth century, but had stabilized by the 1930s, resting on expectations concerning the relevance of physics knowledge at each stage in the manipulation of x-rays. Buying, installing and maintaining equipment, operating this machinery on a daily basis, supervising technicians, interpreting an x-ray picture, administering a dose of x-rays, and ensuring the safety of patients and operators were all seen by this time to require some amount of physics knowledge. Some stages required the input of special knowledge available only to trained physicists, at other stages it was enough for doctors or technicians to have some educational background in physics. Doctors did not share all of the questions of the physicists, but the values of the physicists—quantification, precision, regularity—had increasingly become their own.

2. A Small Band

Bones—Physicists in Medicine— Machines— Societies—Educating Radiologists

"A special word of recognition is due to that very small band of physicists who have foresaken the cold joys of pure intellection to serve sick humanity."[68]

W. Sampson Handley, Surgeon, Middlesex Hospital 1928

2.1 Bones

The first physicists to enter into the world of medicine were few in number, all but ignored by their "brother physicists" who offered them "scant encouragement" (as they would later reconstruct their tale of origins).[69] It is not that x-rays were uninteresting to physicists, but anatomical x-ray pictures were primarily seen as distracting diversions. In a speech addressing the physics of röntgen rays in 1902, Henry Rowland assured his listeners, "we are only considering theory tonight … we do not have to consider the bones, and so on." He went on to complain about the students working in his laboratory: "it was with the utmost difficulty that I kept them from photographing bones."[70] Rowland's

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[68] W. Sampson Handley, "Radiology from a Surgeon's Standpoint," *British Journal of Radiology* 2(1929): 50.

[69] Mayneord, "Introduction," in *History of the Hospital Physicists' Association* (Newcastle upon Tyne: 1983), 2.

[70] Henry Rowland, "The Röntgen Ray, and its Relation to Physics," in *The Physical Papers of Henry Augustus Rowland* (Baltimore: Johns Hopkins Press, 1902), 579.

students weren't the only ones caught up in x-ray fever and his lab was certainly not unique in its obsession with "bones." In the first few months of 1896, before commercial x-ray devices became available, the equipment to make x-ray pictures existed solely in physics laboratories. Doctors wanting to make use of the new technology had to call on a physicist for help. Like Rowland, physicist Arthur Schuster recalled being completely distracted by the medical utility of the new technology:

> My laboratory was inundated by medical men bringing patients, who were suspected of having needles in various parts of their bodies, and during one week I had to give the best part of three mornings to locating a needle in the foot of a ballet dancer, whose ailment had been diagnosed as bone disease. [71]

Schuster even went so far as to take his x-ray apparatus on the road. In one instance, he visited a small town to locate a bullet in the skull of a woman who had been shot by her husband:

> My private assistant completely broke down under the strain and excitement of all this work, and the experiments on the magnetic deflection of kathode rays on which I was then engaged were seriously interfered with by this interruption."[72]

The enthusiastic newspaper coverage of the new x-ray pictures shows a gradual shift of anatomical x-ray work out of physics laboratories and into doctors' offices. On Feb. 7, 1896, *The New York Times* reported on experiments performed by physics faculty at Princeton

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[71] Arthur Schuster, *The Progress of Physics during 33 years (1875-1908)* (New York: Arno Press, 1975), 75.

[72] Ibid.

under the leadership of Prof. W. F. Magie. "Among other things he exhibited a specimen photograph of his own hand …," stating that in his opinion, "it will be of incalculable benefit to the medical profession."[73] Lectures by physics professors on x-rays continued to be newsworthy in the coming months,[74] but even more exciting were stories of the x-rays actually being used for a medical purpose. In February of 1896, *The New York Times* reported that a doctor at St. John's Hospital in New York had asked Prof. W. C. Peckham of the Adelphi Academy in Brooklyn to make a Röntgen photograph for him. A successful photograph was subsequently made of the bones in the patient's wrist and forearm.[75] In Canada, Prof. White and Prof. McLellan, two physicists at the University of Toronto, helped to locate a needle in the foot of a patient at the Grace Hospital. Readers of the *New York Times* were told that "Prof. White of Toronto University made the photographs, and pointed out to the surgeons the exact location of the needle."[76] In June of the same year, a needle in a woman's hand was photographed at Edison's lab, and the patient took the picture back to her physician.[77]

But this initial pattern gradually faded as doctors purchased their own x-ray apparatus. For most physicists, "these stirring times," in Rutherford's words, were soon remembered solely "by the faded X-ray photograph so often seen on the walls of a physical

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73 "The Roentgen Discovery: Prof. Magie's Experiments with the X-Rays at Princeton," *The New York Times*, Feb. 7 1896.

74 "Roentgen Ray Marvels," *The New York Times*, March 7 1896.

75 "Showed Nature's Cure: Bones of a Forearm photographed by the Roentgen Rays," *The New York Times* Feb. 18 1896.

76 "X-Rays to find a Needle in a foot," *The New York Times*, Feb.15 1896.

77 "X-Rays Prove to be Effectual," *The New York Times*, June 20 1896.

laboratory, and viewed with proprietary pride by the professor as the first of its kind in his city or country."[78] By 1897, *The New York Times* was still reporting on successful diagnoses made with x-rays, but the setting of the photographs had shifted. In September of 1897, the paper reported on two successful cases: a 3 year old who had swallowed a hat pin and a 7 year old who had swallowed a melon seed. In both cases, the foreign body was successfully located and in both cases the radiographs were taken by doctors.[79] By the first decade of the 20th century, this shift of anatomical x-ray work out of the physics lab and into the clinic was further justified by a growing uneasiness amongst doctors about the consequences of sending their patients to an "unqualified person" for an x-ray photograph. The hallmark of an unqualified person, according to British physician Dr. Cooper, was anyone totally ignorant of anatomy and surgery.[80] This definition, of course, included physicists.

By 1897, doctors were able to purchase their own x-ray apparatus from companies such as Queen & Co. in Philadelphia, the Electro-Medical Company in Chicago, and K. Schall in London. Physicians outside of major urban centers who lacked access to academic physics labs, were now able to begin experimenting with x-rays. By 1899, testimonials from customers happy with Queen and Company's new self-regulating x-ray tube included doctors in Philadelphia, Pittsburgh and Brooklyn, but also doctors working in smaller

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78 Sir. Ernest Rutherford, "The Development of Radiology," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 13(1920): 147.

79 "Swallowed a Hatpin," *The New York Times*, Sept. 27 1897.; "X-Rays Found the Cause," *The New York Times*, Sept. 27 1897.

80 R. Higham Cooper, "X-Rays in the Diagnosis of Fractures and Dislocations," *The Practitioner: A Medical Journal* (1906).

towns in Alabama, Indiana and Tennessee.[81] With widely available apparatus, doctors no longer sought out the services of their local physicists, and these researchers were free to return to their experimental investigations of the properties of x-rays. But a few physicists remained interested in the medical applications of these rays and a handful found professional posts in hospitals, and later, as teachers in medical schools. These few individuals are the focus of this chapter. Their band was small, but increasingly influential in the developing world of radiology.

The story of the influence of these few physicists who imported their unique values and questions into medicine brings to mind the better-known story of the generation of physicists who played such a prominent role in post-WWII biology. The emigration of physicists into biology following the deployment of the atomic bomb has been well documented and historians have argued over how exactly to understand the influence of these physicists on the development of molecular biology.[82] Francis Crick, for instance, was a physicist-turned biologist who later recalled the galvanizing effect of reading Erwin Schrödinger's *What is Life* (1946).[83] Donald Fleming has argued that Erwin Schrödinger, Max Delbrück and Leo Szilard fostered the right kind of environment for Watson and Crick to make their discoveries. Fleming writes, "By force of numbers and certain messianic qualities, the physicists in the group played the key role in the process by which

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81 Queen & Co. Inc., *Queen Self Regulating X-Ray Tube* (Philadelphia1899).

82 Some historians have argued that the influence of these migrant physicists extended beyond the first generation of molecular biologists. See for instance C.U.M Smith, "Origins of Molecular Neurobiology: The Role of the Physicists," *Journal of the History of the Neurosciences* 14(2005): 214-29.

83 James Watson, "The Double Helix: A Personal Account of the Discovery of the Structure of DNA," ed. Gunther S. Stent (New York: W. W. Norton & Company, 1980), 12.

unfashionable ambitions were stirred up."[84] The primary unfashionable ambition was, of course, the desire to establish the physical basis of life. Evelyn Fox Keller has argued in a similar vein that molecular biology was able to draw on the authority of physics, its techniques and language, but also its attitudes and beliefs. In particular, a "belief in the absolute adequacy not simply of materialism, but of a particular kind of (linear, causal) mechanism; belief in the incontrovertible value of simplicity; belief in the unitary character of truth; and finally, belief in the simultaneous equations between power and knowledge, and between virtue and power."[85] In the following chapters, I will enumerate a slightly different set of values embodied by the first physicists in medicine, most crucially a desire for precision in measurement and a belief in standardisable bodies. Here, I wish simply to establish the existence of this earlier migration of physicists, introducing some of the most prominent individual medical physicists of this early period, mapping their uneven involvement in medical societies and journals and showing the increasingly prominent role that they came to play in radiology as physics teachers beginning in the 1920s. I will argue that it was, at least in part, by taking on these teaching roles that these individuals came be so important in shaping the culture of radiology.

[84] Donald Fleming, "Emigre Physicists and the Biological Revolution," in *The Intellectual Migration: Europe and America, 1930-1960*, ed. Donald Fleming and Bernard Bailyn (Cambridge, Mass.: Harvard University Press, 1969), 156.

[85] Evelyn Fox Keller, "Physics and the Emergence of Molecular Biology: A History of Cognitive and Political Synergy," *Journal of the History of Biology* 23(1990): 406-07.

2.2 Physicists in Medicine

Medical physics is now a well-defined field with societies, journals and formal post graduate training. However, the first professional society for medical physicists in Britain, the Hospital Physicists Association, wasn't formed until 1943.[86] The pattern in the United States was similar. Formal recognition of this branch of physics didn't come until 1947, when the American Board of Radiology instituted a separate diploma in radiologic physics.[87] American medical physicists gained their own professional body, the American Association of Physicists in Medicine in 1959, and an International Organization of Medical Physicists was formed in 1963.[88] The individual physicists introduced in this chapter worked in hospitals and medical schools long before any of these disciplinary institutions were in place. They worked without formal structures to support communication, yet their common background in physics was enough to nurture a shared sense of purpose and allow for the beginnings of a disciplinary vision.

The first two hospitals to hire physicists in Britain did so in the mid 1910s, at a time when there was still a sense among physicists that their discipline was in its professional infancy. Historians have counted 114 academic physicists in the UK in 1900.[89] Most physicists at the beginning of the 20th century were employed in education at all levels, but

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[86] *History of the Hospital Physicists Association*. (Newcastle upon Tyne: 1983).

[87] Edith Quimby, "Medical Radiation Physics in the United States," *Radiology* 78(1962): 521.

[88] J.S. Laughlin and P.N. Goodwin, "Early Organizations of Medical Physicists," *Medical Physics* 25, no. 7 (1998): 1238-43.

[89] Paul Forman, John L. Heilbron, and Spencer Weart, eds., *Physics circa 1900*, vol. 5, Historical Studies in the Physical Sciences (Princeton, NJ: Princeton University Press, 1975), 6.

there was a widespread feeling in the physics community that too many physicists were being created for the available jobs. The landscape improved in 1914 with the creation of the National Physical Laboratory, which historian Russel Moseley has called, the "only oasis in [this] vocational desert."[90] The scarcity of jobs was coupled with a sense of professional invisibility. *Nature* ran a story in 1941 recalling the experience of a young physicist during WWI who had been mistaken for a doctor by the army enlisting committee because one of the committee members had confused "physics" with "physic"[91] With the air of an oft-repeated tale, this anecdote and others like it were useful in calls for the establishment of the Institute of Physics (founded in 1921) as a site for physics research and professional development.

In this landscape, medicine did not provide a substantial source of employment for young British physicists. The first hospital to create a post specifically for a physicist was Middlesex Hospital in London, hiring Sidney Russ in 1913 (Figure 3). By 1920, the Physics Department at Middlesex had several laboratories and boasted a large supply of radium.[92] Russ was educated at University College London, and had worked with Ernest Rutherford in Manchester before coming to Middlesex Hospital on a Beit Fellowship in 1910.[93] He contributed to research on radiation protection and measurement, served as President of

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[90] Russel Moseley, "Tadpoles and Frogs: Some Aspects of the Professionalization of British Physics, 1870-1939," *Social Studies of Science* 7(1977): 431.

[91] Ibid., 433.

[92] "A University Chair of Radiology at Middlesex Hospital," *Archives of Radiology and Electrotherapy* 24(1920): 342-43.

[93] These were medical science fellowships that had only just been established in 1909. "Physics at Middlesex Hospital School: Prof. Sidney Russ, C.B.E.," *Nature* 159(1947): 54.

the Röntgen Society from 1919 to 1920, and was intimately involved in the development of the syllabus and examinations for the first postgraduate program in radiology in England (see below).[94]

The second hospital in England to appoint a physicist was the Royal Cancer Hospital in London. [95] The radiologist there, Dr. Robert Knox invited Charles Edmund Stanley Phillips to be honorary physicist to the hospital (Figure 3). Phillips had grown up in an engineering household: his father's firm had made some of the first marine telegraph cables. Phillips himself was educated privately, and although he studied for a time at the Central Technical College in South Kensington, he did not receive any official degrees. One author of an obituary for Phillips speculated, "It may be that it was owing to his rather unorthodox education that he was so versatile." [96] Before the discovery of x-rays, Phillips had been interested in vacuum tubes and he was one of the founders of the Röntgen Society. He served as President of the society in 1909, and during the First World War was in charge of the X-Ray department at the Royal Herbert Hospital and served as physicist to the X-Ray Committee of the War Office. Later, he became a lecturer at University College London. [97]

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[94] J.E.R, "Obituaries: Professor Sydney Russ," *British Journal of Radiology* 36(1963): 702.

[95] Phillips, "Presidential Address," 16.

[96] R. S. Whipple, "Obituaries: Major C.E.S. Phillips, O.B.E.," *Nature* 156(1945): 228.

[97] Ibid.

Table 1: Physicists working closely with Hospitals or Medical Schools in the UK in the 1920s

Name	**Education**	**Employment Affiliation** (date given is the date of hire)
F.L. Hopwood (1884 – 1954)	DSc. Physics (University College London)	St. Bartholomew's Hospital (1919)[98]
George Kaye (1880 – 1941)	DSc. Physics 1908 (Cambridge)	National Physical Laboratory (1910)[99]
W.V.Mayneord (1902- 1988)	MSc. Physics 1924 (University of Birmingham), [DSc. Physics 1933] (University of Birmingham)	St. Bartholomew's Hospital (1924-1927); Royal Cancer Hospital, London (1927)[100]
Charles E.S. Phillips (1871 – 1945)	No official degrees	Royal Cancer Hospital (191-)[101]
Sidney Russ (1879 – 1963)	DSc. Physics 1905 (University College London)	Middlesex Hospital, London (1913)[102]

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[98] Hopwood was appointed hospital physicist in 1919 but had been teaching medical students since 1911. G. Stead, "Professor Frank Lloyd," *British Journal of Radiology* 27(1954): 317. and J.A.C. Fleming, "Neville Samuel Finzi," *British Journal of Radiology* 41(1968): 552.

[99] Douglas Ambrose, "A History of Kaye and Laby," *Notes and Records of the Royal Society* 60(2006): 49-57. and E.H. Burrows, *Pioneers and Early Years: A History of British Radiology* (Great Britain: Colophon Limited, 1986), 230-31.

[100] F. W. Spiers, "William Valentine Mayneord," *Biographical Memoirs of Fellows of the Royal Society* 37(1991).

[101] Phillips is listed as Hospital Physicist to the Cancer Hospital in 1920 in "Cancer Hospital, Fulham," *Archives of Radiology and Electrotherapy* 25(1920): 335-36. See also Whipple, "Obituaries: Major C.E.S. Phillips, O.B.E.," 228.

[102] J.E.R, "Obituaries: Professor Sydney Russ," 702.

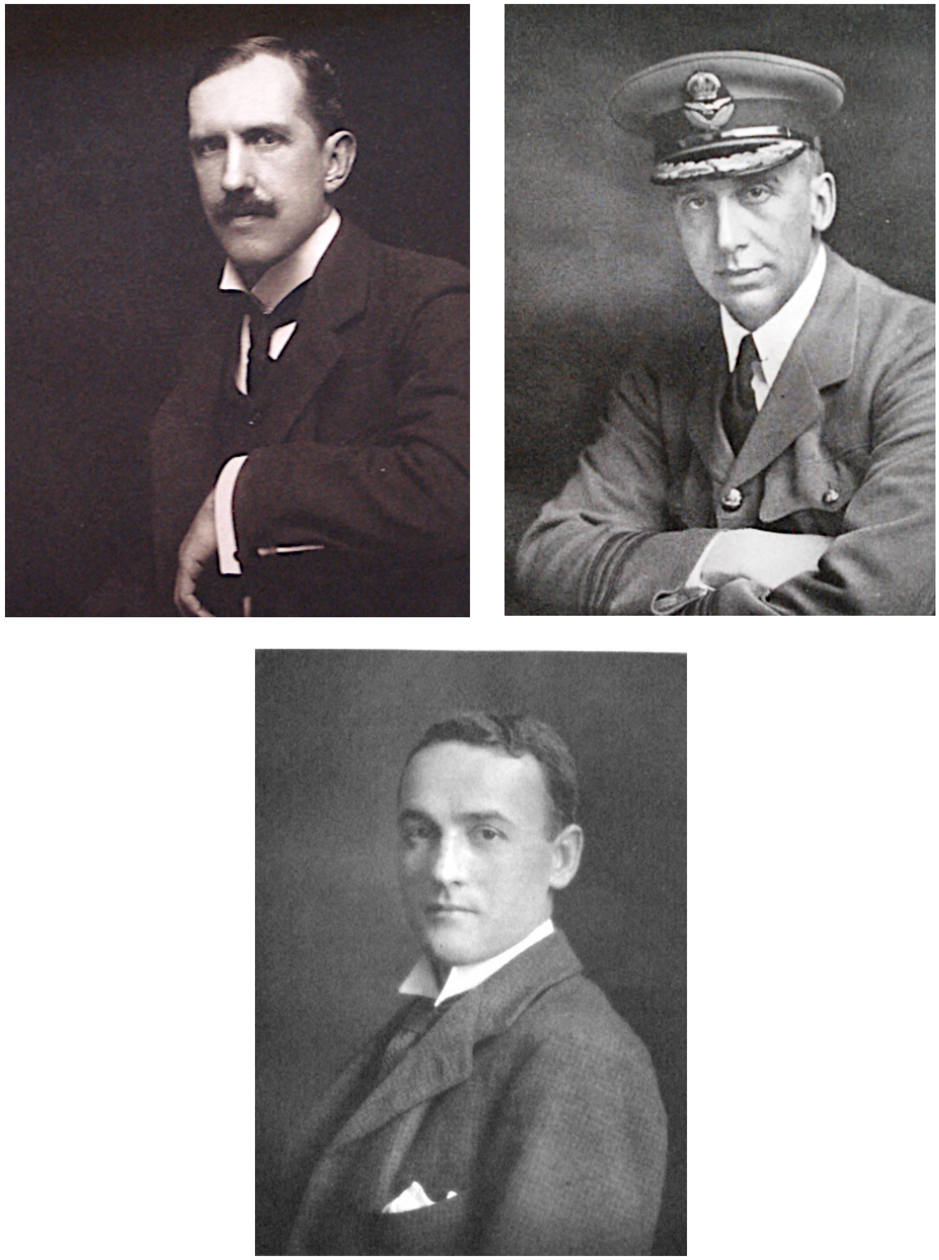

Figure 3: The first British medical physicists. Clockwise from top left: C.E.S Phillips (1871-1945),[103] George Kaye (1880-1941),[104] Sidney Russ (1879 - 1963).[105]

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[103] *Journal of the Röntgen Society* 5 (1908-1909).

[104] *Journal of the Röntgen Society* 14 (1918).

[105] *Journal of the Röntgen Society* 16 (1920).

Perhaps the most prominent British medical physicist from this period was George Kaye (Figure 3). Kaye was a Cavendish-trained physicist who worked with J.J. Thomson. He became involved in medicine in 1913 when he joined a committee of the Röntgen Society charged with establishing a standard measurement for a dose of x-rays (Chapter 3). He went on to become a leading figure in radiology, serving as President of the Röntgen Society in 1917 and President of the British Institute of Radiology in 1929. He was an associate editor for a number of prominent journals including *The Journal of the Röntgen Society* and *The British Journal of Radiology*.[106] His most important leadership role came as Superintendent of the Physics Department of the National Physical Laboratory, a post he took up in 1922. Here, he oversaw safety inspections of hundreds of x-ray departments (Chapter 5).

These three men were some of the most prominent physicists associated with the radiology community in Britain, and there is evidence that even at this early stage, they were identifying their work as a unique and separate branch of physics. In a presidential speech to the Röntgen Society in 1910, C.E.S Phillips felt comfortable referring to 'medical physics' without providing any further explanation. The development of this community of physicists, however, remained slow.[107] In 1922 Kaye estimated that there were only half a dozen physicists working in medicine in Britain (see Table 1). Kaye felt that the average physics student was not receiving any encouragement to think about a career in hospital

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[106] Burrows, *Pioneers and Early Years*: 230-31.

[107] C.E.S Phillips, "Presidential Address," *The Journal of the Röntgen Society* 6(1910): 14.

physics, and, "Even had he been prepared to risk this prospect, he would not have found educational facilities to put him on his way."[108] Hoping to gain support for the creation of more jobs in hospitals for men with his training, Kaye made a case to his colleagues in medicine for the indispensability of the physicist. Implying that British radiology was not reaching its full potential, he painted a picture of a more efficient hospital radiology lab in which the physicist and doctor shared responsibility according to their natural interests and inclinations:

> I fear [the British radiologist] is sadly handicapped by not being able to rely on his brother physicist for the discharge of duties which he has neither time nor, possibly, the inclination, to see to himself … If the Royal Society of Medicine is prepared to give whole-hearted support to these ideas of the future cooperation of the medical man and the physicist, it will have to use its great influence to secure the appointment of part or full- time physicists to the various hospitals, and further, to ensure that the Universities and other teaching centers put themselves into a position to provide physicists with courses of instruction calculated to turn out men of the right calibre and training."[109]

This vision would become a reality, but not for many more decades.

In the United States, at the turn of the twentieth century, the landscape was similar. In 1899, Henry Rowland addressed his fellow physicists at the American Physical Society in

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[108] G.W.C Kaye, "Radiology and Physics," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* (1922): 34.

[109] Ibid.

New York proclaiming: "we form an aristocracy, not of wealth, not of pedigree, but of intellect and of ideals …."[110] This kind of rhetoric was a rousing call to arms for a profession that had only recently established its own journal, the *Physical Review* (first published in 1893). But it was also a justification or at least explanation for a community with very small numbers. Daniel Kevles estimates that in the 1890s, there were only 200 American physicists, and of those, only 40 regularly published.[111] Given these small numbers, it is unsurprising, that were only a few American physicists working in a medical context in the early 20th century.

The man who was later remembered as being "most largely responsible for the growth in medical radiological physics as a profession in the United States," was Gioacchino Failla.[112] Failla was hired by Memorial Hospital in New York City in 1915 to supervise the production of radon, and was in fact an electrical engineer by training, only pursuing a doctorate in physics later in his career. He was born in Sicily, and immigrated to New York in 1906, receiving an electrical engineering degree from Columbia University. After working at Memorial hospital for a number of years, he chose to pursue a doctorate in physics, and in 1923 he completed a DSc. at the University of Paris under Marie Curie. In addition to overseeing the laboratory at Memorial Hospital, Failla was later involved in

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[110] Henry Rowland, "The Highest Aim of the Physicst," in *The Physical Papers of Henry Augustus Rowland* (Baltimore: The Johns Hopkins Press, 1902), 668.

[111] Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Alfred A. Knopf, 1979), 90.

[112] Edith Quimby, "Gioacchino Failla (1891 - 1961)," *Radiology* 78(1962): 643.

radiation safety on the Manhattan Project. By 1962, Failla's lab was remembered as foundational to the field of medical physics:

> In less than half a century medical radiologic physics has grown from a tiny sprout to a many branched tree. The sprout grew in the first Failla laboratory at Memorial Hospital, and most of those who grace the tree are in some sense his spiritual descendants.[113]

These were the words of Failla's colleague, Edith Quimby who worked with him for many years at Memorial Hospital. Quimby received a BSc from Whitman College in Walla Walla, Washington and an MSc from UC Berkeley. She started at Memorial Hospital in 1920 and worked there for over two decades. Her alma mater, Whitman College, awarded her an honorary doctorate in 1940, and beginning in 1942, she taught physics to radiologists at Columbia University. She was instrumental in establishing certification for radiological physicists through the American Board of Radiology. [114]

By the early 1920s, the radiation research lab at Memorial Hospital was not the only medical center employing physicists (see Table 2). In 1921, Philadelphia General Hospital hired James L. Weatherwax to be its resident physicist.[115] Besides his work at the hospital, Weatherwax taught at the Graduate School of Medicine at the University of Pennsylvania

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113 ———, "Medical Radiation Physics in the United States," 521.

114 Harold Jacobsen and Rosalyn Levine, "Im Memorium: Edith Quimby," *Radiology* 147(1983): 290. and Gioacchino Failla, "Edith Quimby Honored," *Radiology* 35(1940): 104-05.

115 Quimby, "Medical Radiation Physics in the United States," 518.

Table 2: Physicists working in Hospitals or Medical Schools in the US (1920s)

Albert Bachem	DSc. Physics	University of Illinois College of Medicine (192-)[116]
William Duane (1872 – 1935)	DSc. Physics 1897 (University of Göttingen)	Harvard University, Professor of Biophysics (1917) and Physicist to the Cancer Commission (1927)[117]
Gioacchino Failla (1891 – 1961)	E.E. (Columbia), DSc. Physics 1923 (University of Paris)	Memorial Hospital, New York (1915)[118]
Wilhelm Stenstrom (1891 – 1973)	DSc. Physics (University of Lund)	State Institute for Study of Malignant Disease, Buffalo (1921); University of Minnesota Cancer Institute (1926)[119]
Otto Glasser (1895 – 1964)	DSc. Physics 1919 (University of Freiberg)	Cleveland Clinic (1923)[120]
Edith Quimby (1891 – 1982)	MSc. (Berkley)	Memorial Hospital, New York (1920)[121]
Lauriston Taylor (1902 – 2004)	A.B. 1926 (Cornell)	National Bureau of Standards (1927)
James L. Weatherwax	A.M. 1916 (University of Missouri)	Philadelphia General Hospital (1921) University of Pennsylvania Graduate School of Medicine (1922)[122]

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[116] *1927 Graduating Class, University of Illinois College of Medicine*, (University of Illinois at Chicago Library, 1927).

[117] Duane started as Professor of Physics at Harvard in 1913. His title was changed to Professor of Biophysics in 1917. P.W. Bridgman, *Biographical Memoir of William Duane*, ed. National Academy of Sciences (1936).

[118] Quimby, "Gioacchino Failla (1891 - 1961)," 643. J.A. del Regato, "Gioacchino Failla," *International Journal of Radiation Oncology Biological Physics* 19(1990): 1609-20.

[119] "Karl Wilhelm Stenstrom," *Radiology* (1974): 494.

[120] Lauriston Taylor, "Otto Glasser," *Radiology* 84(1965): 958 – 59. and Marvin Williams, "Otto Glasser (1895 - 1964)," *Radiation Research* 31(1967): 180-82.

[121] Jacobsen and Levine, "Im Memorium: Edith Quimby," 290. and Failla, "Edith Quimby Honored," 104-05.

[122] Stanton, "James L. Weatherwax: Pioneer in the Physics of Radiology," 332-35.

beginning in 1922, and published a textbook for radiology students, *Physics of Radiology* in 1931.[123] Otto Glasser was another leader in the field. He received his PhD in physics from the University of Freiburg in 1919, moved to the United States in 1922, and started work at the Cleveland Clinic in 1923. Like many of these first physicists in medicine, he, too, took on a teaching role, at the Post-Graduate Medical School at Columbia. Glasser developed one of the first American clinical dosimeters using the new röntgen unit, the Victoreen r-meter.[124] William Duane, another noteworthy member of this group, was the first in America to hold a professorship in biophysics. He earned his PhD from the University of Berlin in 1897 and took up a professorship at Harvard University in 1913. He served on the international committee that established the röntgen unit.[125] Finally, Lauriston Sale Taylor, was hired by the National Bureau of Standards in 1927 and oversaw the creation of the first national standards in the United States for the safe use of x-rays (Chapter 5).

2.3 Machines

In the same way that these physicists only slowly came to be regarded as experts over x-ray measurement and safety, they were not immediately regarded as the clear experts over the apparatus itself. The x-ray tubes employed in medicine were symbolic of physics, but they were also symbolic of the new specialty of radiology, and the first doctors

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[123] Leonard Stanton, "James L. Weatherwax: Pioneer in the Physics of Radiology," *RadioGraphics* 6(1986): 332-35.

[124] Taylor, "Otto Glasser," 958-59. and Williams, "Otto Glasser (1895 - 1964)," 180-82.

[125] "William Duane. In Memorium," *Radiology* 24(1935): 372.

to experiment with x-rays were justifiably proud of their intimate knowledge of their machines. In 1920, Rutherford described x-ray tubes as objects of reverence and inspiration:

> When we consider the remarkable potentialities of the x-ray tube, whether as an aid to medicine and surgery or as one of the most powerful means of unraveling the mysteries of the constitution of atoms and molecules, it is not a matter of surprise that to a man of scientific imagination an X-ray tube in action excites almost a feeling of mystery and constitutes a never-failing stimulation of scientific interest. Possibly for this reason the devotees of radiology exhibit to an unusual degree an infectious enthusiasm and faith in their subject, and an unflagging optimism for its future."[126]

Radiologists may have identified their apparatus as a means of "unraveling the mysteries" of the universe, and therefore as enigmatic objects of science, but at the same time these machines were the single distinguishing feature of their practice and crucial to their distinct identity within medicine. The first doctors to use x-rays were drawn to the new technology because of their interest in electric devices, and many of these doctors derived real joy from 'tinkering' with their equipment. In 1919, English physician Dr. Barclay invoked the image of radiologist-as-tinkerer:

> In no other branch [of medicine] is there room for such thought and ingenuity in the tools with which the work is done, and this factor has attracted some of our ablest workers. Nearly all our apparatus has been designed by, or built to the instructions

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[126] Rutherford, "The Development of Radiology," 147.

> of, medical men … It is a delight to see the apparatus in some hospitals and private rooms – pure make-shift devices, often made for a few pence, out of some piece of scrap, and yet performing the work for which it was designed with perfect efficiency.”[127]

Delighting in this ingenuity, Barclay gave the example of a packing case that made an excellent x-ray table at a base camp in France during the war. The early x-ray journals in both the United States and Britain are full of reports of modifications and inventions made by doctors. In the very first pages of the first volume of *The American X-Ray Journal* in 1897, readers were told of an “ingenious device” developed by Dr. Herber Robarts of St Louis. He had designed a small button for the handle of his fluoroscope which allowed him to turn on and off the current to the x-ray tube at will.[128]

Many of the first innovations were doctor-driven, but one area in which physicists did play a large role was in tube design. In fact, the two most important innovations in this area were introduced by physicists, in the period before any physicists were working in a medical setting. In the estimation of the engineers at the Kny-Sheerer company in 1905, “Only in one particular has pure physics suggested anything really new since the original discovery [of x-rays] was made …”[129] These authors were referring to the invention of the focus tube. The original cathode ray tube used by Röntgen, shown in Figure 4, produced a

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[127] A.E. Barclay, "President's Address: Ideals in Radiology and Electrology," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 13(1920): 2.

[128] Arthur Aver Law, "The Use of the X-Rays and Fluoroscope in Surgery," *American X-Ray Journal* 1(1897): 6.

[129] Kny-Sheerer Company, *Roentgen X-Ray Apparatus and Accessories*: 5.

broad beam of x-rays where the cathode rays hit the glass on the left hand side. The focus tube, on the other hand had a concave aluminum mirror as the cathode and a platinum disc as the anode (Figure 5). This allowed for a focused beam of cathode rays to create a similarly focused beam of x-rays. This innovation was announced by Roentgen with his colleague Zehnder in 1896.[130] Much sharper radiographs could be made with this new type of tube, prompting many to argue that with this innovation, "the Roentgen system may be said to have begun."[131]

The next significant invention was the hot cathode or Coolidge tube. Rutherford called this product of physics research "an embodiment of many of the principles brought to light in some of the most notable investigations of our age."[132]

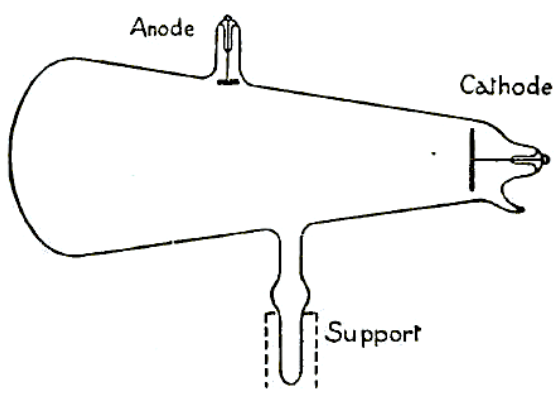

Figure 4: Cathode-ray tube of the kind used by Röntgen. X-rays were created on the left side where the cathode rays hit the glass.[133]

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[130] Kaye gives credit for the first focus tube to Prof. Herbert Jackson of King's College London, a professor of chemistry. G.W.C Kaye, *X-Rays*, 2nd ed. (London: Longmans, Green &Co., 1917), 31.

[131] Kny-Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories* (New York1905), 6.

[132] Rutherford, "The Development of Radiology," 147.

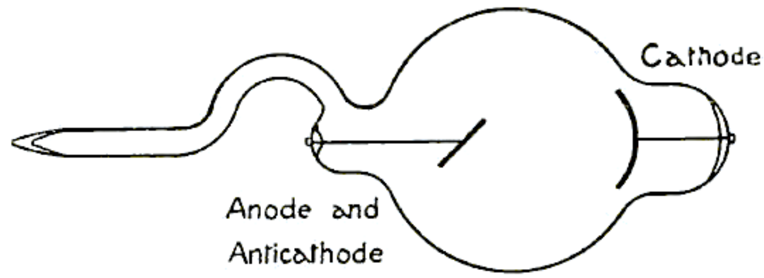

Figure 5: A simple focus tube. X-rays are created at the anti-cathode.[134]

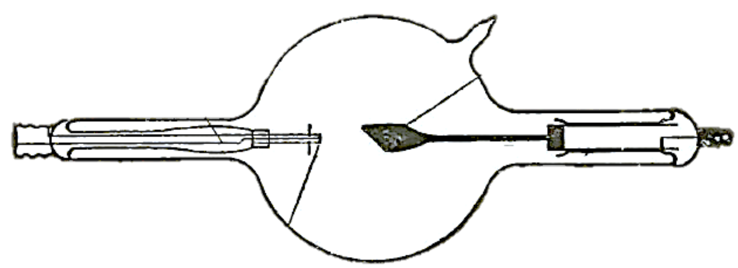

Figure 6: A Coolidge Tube[135]

This tube was invented by William Coolidge, a physicist working at the industrial research laboratory at General Electric.[136] William Coolidge received a BSc in 1891 from

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[133] G.W.C Kaye, *X-Rays*, 4th ed. (London: Longmans, Green and Co., 1923), 29.

[134] Ibid., 31.

[135] Ibid., 44.

[136] The first hot-cathode tube was invented by Julius Lilienfeld in Germany, but his design relied on the presence of a small amount of gas in the tube. The first US patent for a truly high-vacuum tube was filed by Coolidge in 1913. See Robert G. Arns, "The High-Vacuum X-Ray Tube: Technological Change in Social Context," *Technology and Culture* 38(1997): 852-90.

MIT in physical chemistry and electrical engineering and a PhD in physics in 1899 from the University of Leipzig. He returned to MIT for a few years before taking a job at the newly formed General Electric Research Lab in 1905.[137] Here, he developed the first incandescent bulbs with a ductile tungsten filament, marketed in 1911. [138] Coolidge also experimented with using a tungsten cathode in an x-ray tube with a high vacuum. These investigations were quickly successful and he announced his new tube design in a paper in 1913.[139] The new x-ray tube differed from older designs by having a hot tungsten cathode that emitted electrons through thermionic emission at a much higher vacuum (a few hundredths of a micron rather than a few microns).[140] These were manufactured on a small scale with 3000 sold the first year and 6000 in 1914.[141] The initially low sales increased once the military began ordering tubes for portable x-ray units in WWI.[142]

The two physicists involved in these two innovations in tube design, Röntgen and Coolidge, did not work in a medical setting and the physicists in medicine listed in Tables 1 and 2 were not directly involved in equipment design. But as the Coolidge tubes began to be used in hospitals, medical physicists were called on to help as doctors learned how to

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[137] GE was founded in 1892 and establised its research lab in 1900. Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (Cambridge: Cambridge University Press, 1985), 1985.

[138] E. Dale Trout, "ObituaryL William David Coolidge 1873-1975," *The British Journal of Radiology* 48(1975): 1050.

[139] W. D. Coolidge, "A Powerful Röntgen Ray Tube with a pure electron discharge," *Physical Review* 2(1913).

[140] Ibid., 414.

[141] Reich, *The Making of American Industrial Research*: 88.

[142] Ibid., 89.

use the new tubes. In 1915, Sidney Russ was asked by Dr. Lyster at the Middlesex Hospital to do some tests on their newly acquired Coolidge tube before it was put into clinical use.[143]

2.4 Societies

In 1931, the British physician A.E. Barclay talked about the space carved out by radiology between physics and medicine: "We have our own journal; that is inevitable, for many of our contributions lie on the borderland—they are often too physical for publication in the medical and far too medical for the physics journals."[144] It was precisely because the interests of radiologists seemed "too physical" for many other doctors, that the first x-ray doctors loudly proclaimed the need for medical training in interpreting radiographs. These doctors were adamant that X-ray diagnosis be performed by an individual with a medical degree. As a result, in both the United States and Britain the presence of physicists in professional radiological societies was a source of tension.

In the United State, physicists were initially welcome but then pushed to the periphery of x-ray societies. The first x-ray society in North America, the Roentgen Society of the United States, held its inaugural meeting in December of 1900 in New York.[145] In

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143 Sidney Russ, "Measurement of the Radiation from the Coolidge and Other X-Ray Tubes in Clinical Use," *Journal of the Röntgen Society* 11(1915): 42-50.

144 A.E. Barclay, "The Dangers of Specialization," *The British Journal of Radiology* (1931): 78.

145 "Roentgen Society of the United States," *The American X-Ray Journal* 7(1900): 741-42.

order to be inclusive of Canadian physicians desiring membership, the name was changed in 1902 to the American Roentgen Ray Society.[146] Beyond the admonition that "No quacks or fakes of whatever sort need apply," there weren't firm criteria for membership.[147] Members could be:

> physicians and surgeons, dentists, investigators, authors on x-ray topics, inventors, radiographers, or their assistants in hospitals, military or State institutions, technical electricians, chemists, teachers of chemistry and physics, specialists and experts in electro-technique, qualified by at least one year in experience with radiant matter, its application or therapeutic use.[148]

Potential members submitted their names along with a $5 application fee, and their credentials were vetted by a committee. Some of the first members suggested that the society should become a subcommittee of the American Medical Association but this would have imposed a strict requirement that all members possess a medical degree. Ultimately, this restriction to medical members was deemed undesirable by the founders of the American Roentgen Ray Society who, "wish[ed] to enroll the entire profession, all the honest workers in the x-ray field under one banner."[149] In particular, "since the x-rays

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146 Edward Skinner, "The Organization of the Roentgen Society of the United States," in *The American Roentgen Ray Society 1900-1950* (Springfield, Ill: Charles C Thomas, 1950), 6. A separate Canadian Association of Radiologists was formed in 1937. This was the same year that radiology was recognized as a distinct specialty by the Royal College of Physicians and Surgeons of Canada. Charles Hayter, "Radiation Oncology in Canada 1895-1995," *International Journal of Radiation Oncology* 36(1996): 489.

147 "Roentgen Society of the United States," 753.

148 From the Consitution of the Roentgen Society of the United States, *The American X-Ray Journal* 6 (1900), reproduced in Skinner, "The Organization of the Roentgen Society of the United States," 8.

149 "Roentgen Society of the United States," 779.

contemplate radiant matter we need the assistance of physicists in our meetings, which could not be, as members of the American Medical Association."[150] The leadership of the society was, however, firmly in the hands of the medical community. The first president, Herber Robarts, was a physician, and of the first 5 executive officers elected, 4 were doctors and 1 was a dentist.[151] The Societies' committees on Publication and on Conduct were composed entirely of doctors and dentists.[152] The second president, elected in 1901, was also a physician, Dr. G.P. Girdwood of Montreal. In that same year, Arthur W. Goodspeed, a physicist, was elected one of two vice-presidents, and he became president the following year. Arthur Goodspeed was, however, the only physicist ever to hold one of the executive posts in this society.[153] In 1902, there were 222 members of the society, 39 of whom did not possess an MD or a DDS (less than 18%).[154] By 1908, this percentage had dropped to 10.5 % (171 members, 18 non-medical members).[155]

In 1911, the society amended its requirement for membership. In order to be a full, voting member, the "Applicant must have been a graduate in medicine and have been actively engaged in x-ray work for at least two years after his graduation." [156] There were

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[150] Herber Robarts, "President's Address," *The American X-Ray Journal* 7(1900): 807.

[151] "The First Annual Meeting of the Roentgen Society," *The American X-Ray Journal* 7(1900): 805-06.

[152] "Roentgen Society of the United States," 753-54.

[153] The list of executive officers for each year is available in the *Transactions of the American Roentgen Ray Society*.

[154] *Transactions of the American Roentgen Ray Society* (1902): 7-12.

[155] *Transactions of the American Roentgen Ray Society* (1908): 313-317.

[156] "American Roentgen Ray Society: Proceedings of the Twelfth Annual Meeting," *American Quarterly of Roentgenology* 3(1911): 210.

other, non-voting levels of membership, but this change in the requirements for membership relegated physicists to the sidelines of the society. Looking back on this history, Edith Quimby seems justified in her opinion that the American Roentgen Ray Society, "was never really much interested in the physicist."[157] The American Radium Society, founded in 1916, was friendlier to physicists, and admitted Gioachianno Failla as a full member in 1918.[158] The Western Roentgen Society, formed in 1915, was envisioned entirely as a medical society, and its executive officers were all doctors.[159] This society changed names to The Radiological Society of North America and by the late 1920s, there were a number of physicists in the society, but still as associate, rather than full members. Edith Quimby remembered that the physicists, "were welcomed at the meetings, but their own place on the programs was at first usually on Friday afternoon after everyone else had gone home and they could talk to each other."[160]

In Britain, physicists had a much more stable home in the Röntgen Society, but their perceived dominance in that society caused a large group of doctors to split off and form their own wholly medical organisation. The Röntgen Society was then formed in 1897, and was the first society to deal exclusively with x-rays in Britain. The Society welcomed medical and non-medical members, the only requirement being, "That candidates for

[157] Quimby, "Medical Radiation Physics in the United States," 521.

[158] Ibid.

[159] *Journal of Roentgenology* 1 (1918): 450-451.

[160] Quimby, "Medical Radiation Physics in the United States," 521.

membership must have shown some scientific interest in the rays."[161] There are conflicting reports about the relative importance of the medical and physical constituencies in founding this society. In 1931, Dr. Barclay gave full credit to the physicist, saying that, "It was Silvanus Thompson … who first made the medical man feel the necessity for a society where he could meet the physicists, and others interested, to discuss the properties of the new rays and to see how the discovery could best be harnessed to the service of medicine."[162] But in a much earlier account from 1904, J.J. Vezey recalled the impetus coming from a small group of doctors who then invited Thomson to be the first president of the society.[163] Either way, it is clear that from the founding of the society, doctors took a back seat. In addition to Silvanus Thompson, who was President, there were 5 Vice Presidents in the first Council of the Röntgen Society, and of those 5, only 1 was a doctor.[164] The medical applications of the x-rays were only one small part of the stated interests of the society as a whole, which included research into the nature of the rays, work on improving the apparatus, and work on both medical and non-medical applications of x-rays.[165]

The first doctors involved in the Röntgen Society worried about discussing sensitive or confidential medical cases in front of a lay audience, and there was talk in 1897 of

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[161] "The Roentgen Society," *Archives of the Roentgen Ray and Allied Phenomena* 2(1897): 5.

[162] Barclay, "The Dangers of Specialization," 60; Rutherford, "The Development of Radiology."

[163] J.J. Vezey, "The Röntgen Society: Its Past Work and Future Prospects," *Journal of the Röntgen Society* 1(1904): 2-3.

[164] "The Roentgen Society," 5.

[165] "Editorial," *Archives of the Roentgen Ray* 2(1897): 39.

forming a separate medical section of the society.[166] This didn't happen right away and the dissatisfaction of a number of doctors grew until they broke off to form a wholly medical British Electrotherapeutic Society in 1902. This society later united with 22 other medical groups to form the Royal Society of Medicine in 1907, becoming thereafter the Electrotherapeutic Section of that society.[167] As a wholly medical society, physicists were explicitly excluded. The Röntgen Society continued, however, to have a robust membership that was fairly evenly split between medical and non-medical members.[168] A snapshot of new members elected to the Röntgen Society in March, April and May of 1918 shows that of 31 new members, there were 16 doctors, 8 radiographers, 4 physicists, 2 engineers and 1 dentist.[169]

In 1917, a third society was formed with explicitly political aims. The British Association of Radiology and Physiology (BARP) was formed to serve the interests of radiology as a profession. It was this group that oversaw the formation of the Cambridge Diploma in Radiology (see below).[170] This group was almost entirely medical. In 1919, there were 166 members, all with medical degrees, except for 4: Lachlan Gilchrist (no

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166 ———, "The Röntgen Society: Its Past Work and Future Prospects," 2.

167 Burrows, *Pioneers and Early Years*: 175-76.

168 "List of Members," *Journal of the Röntgen Society* 2 (1905-1906): 16-19.

169 "New Members Elected by Ballot," *Journal of the Röntgen Society* 14(1918): 99-101.

170 Burrows, *Pioneers and Early Years*: 178-85.

degrees listed), Frederick James Harlow (BSc, ARCSc, D.I.C.), Edith Stoney (DSc.) and Sidney Russ (DSc.).[171]

In 1923, the BARP became the British Institute of Radiology (BIR), and in 1927, the BIR amalgamated with the Röntgen society, in the wedding scene with which this story opened. This reunion of the medical and non medical factions was the result of 2 years of negotiation requiring “patience and long drawn-out discussions.”[172] The British Institute of Radiology was “to serve as a meeting place of all interested in the subject—medical men of every department, radiologists, physicists, technicians or radio-graphers, instrument makers and manufacturers”[173] By 1927, it had more than 300 active members.[174]

2.5 Educating Radiologists

By the late 1920s, medical physicists in both the United States and Britain were beginning to occupy a clearly demarcated space in their respective radiological communities. In the United States, they were welcomed at the meetings of the Radiological Society of North America, and in Britain, they were members of the British Institute of Radiology, alongside doctors. And by this time, physics had become a central part of the education of young radiologists. When the first postgraduate programs in radiology were

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[171] *Archives of Radiology and Electrology* 24 (1919): 62-63.

[172] Rolleston, "Address at the Amalgamation of the Röntgen Society and the British Institute of Radiology," 5.

[173] Ibid., 2.

[174] Ibid., 7.

developed in the late 1910s and early 1920s, the physics of x-rays formed a substantial part of the curriculum. This provided a stable social role for medical physicists as teachers.

By the early 20th century, in both the United States and Britain, it was deemed desirable for medical students to have some exposure to physics either in their undergraduate or pre-medical education. In fact, rhetoric stressing the foundational nature of physics to medicine stretches back into the mid-19th century. Claude Bernard, one of the leading voices in the turn to laboratory-based medical knowledge, argued in 1865 that, "physics and chemistry serve as powerful instruments for physiology"[175] Others made the epistemic relationship more clearly hierarchical. In 1898, Daniel Gilman envisioned medicine as an application of physics (which itself was applied mathematics): "Medicine and surgery are based upon pathology, pathology rests on physiology, physiology upon chemistry, chemistry upon physics, and physics upon mathematics."[176] The physicist Henry Rowland agreed, arguing, "The human body is a chemical and physical problem, and these sciences must advance before we can conquer disease."[177]

These kinds of declarations motivated the inclusion of physics in medical and pre-medical curriculum, as well as the creation of physics text books aimed directly at medical students. The term 'medical physics' first appeared in an early American text, *A Text-book of Medical Physics* (1885) by John C. Draper. Draper lamented that doctors in the United

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[175] Claude Bernard, *An Introduction to the Study of Experimental Medicine*, trans. Henry Copley Greene (New York: Dover Publications Inc., 1957), 94.

[176] Gilman, *University Problems in the United States*: 230-31.

[177] Rowland, "The Highest Aim of the Physicst," 678.

States had yet to appreciate the full importance of physics to medicine: “The fact that a knowledge of Physics is indispensible to a thorough understanding of Medicine has not yet been as fully realized in this country as in Europe.”[178] He defined his subject in the following way:

> Medical physics discusses the laws and phenomena of physics, with which the physician should be acquainted, to understand the processes of life, and also those, a knowledge of which is necessary for the improvement of the hygienic condition of the community in which he lives.[179]

His vision of the applicability of physics was expansive: “there is scarcely a principle of physics which is not applied in some form in the human body.”[180] He argued that the laws of hydraulics, for instance, should be applied to better understand the circulation of fluids in the body. These laws were also crucial to a complete understanding of sanitation. In addition, Draper pointed out that surgical apparatus were dependent on levers and wedges, and that a thorough understanding of vision must appeal to the laws of optics just as hearing depends on the laws of acoustics. Draper devoted only the last 100 pages of his 600 page book to electricity and electrotherapy.

Draper’s text was reviewed quite favourably in the *Journal of the American Medical Association*: “We cannot too earnestly hope that this, the first work on medical physics in

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[178] John C Draper, *A Text-book of medical physics: for the use of students and practitioners* (Philadelphia: Lea Brothers & Co., 1885), v.

[179] Ibid., 36.

[180] Ibid.

this country, is the forerunner of a new era in medical education. Thus far our colleagues may, with but few exceptions, be said to have 'thrown physics to the dogs.'"[181]

Physics did not become a standard part of the undergraduate medical curriculum in the United States, but it was increasingly seen to be a crucial element of a good premedical education. In his 1910 report on the sorry state of American medical schools, Abraham Flexner referred to, "The essential dependence of modern medicine on the physical and biological sciences ...,"[182] but Flexner did not envision formal physics training to be a part of his ideal medical curriculum. He advised that medical schools should require for admission "at least a competent knowledge of physics, chemistry and biology …"[183] For Flexner, these were the pre-medical sciences that formed the basis of the medical sciences (physiology, pathology, etc.).

In Britain, the existence of physics textbooks aimed explicitly at medical students speaks to a more conscious effort to include physics in undergraduate medical training. In *A Text-Book of Physics: With Section on the Application of Physics to Physiology and Medicine* (1902), the author, RA Lehfeldt,[184] justified his subject with an appeal to the reductionist claim that physiology was nothing more than physics:

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[181] "Review: A text-book of medical physics," *Journal of the American Medical Association* VII, no. 3 (1886): 83.

[182] Abraham Flexner, *Medical Education in the United States and Canada: A Report to the Carnegie Foundation For the Advancement of Teaching* (New York: Arno Press & The New York Times, 1972), 54.

[183] Ibid., 61.

[184] Lehfeldt had a DSc. and was a professor at the East London Technical College.

> This book has been written from the point of view of the student of medicine, in the hope of attracting attention to the intimate dependence of physiology on physical principles … containing so much physics as the student among his many other claims can find time for.[185]

The book included chapters on heat, fluids, sound, chemistry, electricity and light (but oddly no section on x-rays). *Elements of Physics for Medical Students* (1907) written by Frederic James M. Page,[186] was motivated with reference to the practical need for physics. Page wanted his book to "bring into prominence those practices of physics which might be useful to the student in his future career."[187] Towards this end, Page emphasized hands-on experience, giving exercises in which students were asked, for instance, to: "Couple up five cells of a Grove battery with a large Ruhmkorff coil and compare the discharge: a) in air b) in a partial vacuum c) in a Geissler tube d) in a Röntgen tube."[188] The authors of these textbooks weren't included in the list of medical physicists in Table 1, as these individuals weren't primarily involved in x-ray research or in the education of radiologists in particular. Lehfeldt and Page certainly corroborate the impression, however, that many physicists were involved in education at the turn of the century, and both should be included in a longer list of the physicists who migrated into medicine in the early 20th century.

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[185] R.A Lehfeldt, *A Text-Book of Physics: With Sections on the Application of Physics to Physiology and Medicine* (London: Edward Arnold, 1902), preface.

[186] Lecturer on Physics and Chemistry at the London Hospital Medical College.

[187] Frederic James M. Page, *Elements of Physics for Medical Students* (London: Cassell and Company, Limited, 1907), v.

[188] Ibid., 269-70.

British physicists continued to write textbooks for a medical audience into the second and third decades of the twentieth century. W. H. White, a Lecturer in Physics at the East London College and St. Mary's Hospital Medical School wrote *A Handbook of Physics* (1912). Hugh Candy, an instructor as the London Hospital Medical College published *A Manual of Physics Theoretical and Practical for Medical Students* (2nd Ed. 1918) and G. Stead, Reader in Physics at Guy's Hospital Medical School, London wrote *Elementary Physics for Medical, First Year University Science Students and General Use Students* (1924). Richard Ablett, a Lecturer in Physics at the University of Liverpool hoped to remedy his observation that most doctors "start their medical career with little or no knowledge of physics …"[189] with his text, *A Course in Physics for Medical and Dental Students* (1930).[190]

Physicists with stronger ties to the x-ray community published textbooks as well. Sidney Russ wrote *Physics for Medical Students* (1928) for students taking pre-medical examinations in physics. He wanted in particular "to indicate throughout the text some direct applications of physics in medicine, hoping in this way to create an interest among medical students in a subject which is often rather disliked."[191]

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[189] G. Stead, *Elementary Physics: For Medical, first year University Science Students and General Use in Schools* (London: J&A Churchill, 1924), v; ibid.

[190] Richard Ablett, *A Course in Physics for Medical and Dental Students* (London: Oxford University Press, 1930).; Hugh C. H. Candy, *A Manual of Physics: Theoretical and Practical for Medical Students*, 2nd ed. (London: Cassell & Company, Ltd. , 1918).; Lehfeldt, *A Text-Book of Physics: With Sections on the Application of Physics to Physiology and Medicine*.; Page, *Elements of Physics for Medical Students*.; Stead, *Elementary Physics: For Medical, first year University Science Students and General Use in Schools*.; W.H. White, *A Handbook of Physics* (London: Methuen & Co Ltd., 1912).

[191] Sydney Russ, *Physics for Medical Students* (Edinburgh: E&S Livingstone, 1928), v.

Textbooks written for radiologists in particular almost always included large sections of physics. The first quarter of the *X-Ray Manual* written for the United States Army was comprised of a section devoted to x-ray physics, written by a physicist John Shearer, who later sounded the alarm on the inadequacy of commercially available safety devices for x-rays (Chapter 5). The *X-Ray Manual* was first published in 1917 and reprinted 7 times before 1925. It placed a heavy emphasis on the particulars of the equipment used in x-ray wok, "with the hope that their study might enable the roentgenologist to prepare for service in less time and be better able to utilize the apparatus with which he is compelled to work."[192] The text was well reviewed in the *American Journal of Roentgenology* for highlighting "numerous points in practical technical roentgenology." The physics section was praised in particular: "This chapter is not written in the usual dry didactic fashion found in the average textbook, but it is evidently written with the object of making a firm impression on the minds of the readers who some time may find it necessary to be practical electricians as well as physicians."[193]

Other books written by physicists for radiologists focused solely on the physics of x-rays. George Kaye's *X-Rays* (1914, 1917, 4th Ed, 1923) was entirely devoted to a review of current physics research. *X-Rays and X-Ray Apparatus: An Elementary Course* (1924)

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192 American Roentgen Ray Society, *X-Ray Manual: U.S. Army* (New York: L. Middleditch Co. Printers, 1917), 5.

193 Isaac Gerber, "Book Review," review of United States Army X-Ray Manual, *American Journal of Roentgenology* 5(1918): 604.

written by John K Robertson, a Professor of Physics at Queen's University in Kingston similarly stressed the physical principles underlying the use of x-rays.[194]

Textbooks written specifically for radiologists, rather than a more general medical audience, were motivated by the postgraduate programs in radiology that were instituted after WWI. Much of the impetus for this kind of separate training for doctors wanting to specialise in x-ray work came from the First World War. Figure 7 shows a roentgenologist working with a surgeon at an American military hospital in 1918. The picture depicts the division of labour supported by American roentgenologists who consistently argued for their special expertise. In their opinion, it was useless for a surgeon to try to don a fluoroscope himself. Roentgenologists emphasized the special training that was required to see and interpret an x-ray image, and in this case, localize a foreign body in three dimensions. But the dream that every x-ray operator would be a specially trained doctor, or even a doctor at all, was difficult to achieve in practice. During the war, the demand for x-rays to locate bullets and shrapnel in wounded soldiers increased dramatically, and there simply weren't enough qualified doctors to oversee all of the x-ray installations in use in military hospitals.

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[194] Kaye, *X-Rays*.; John K. Robertson, *X-Rays and X-Ray Apparatus: An Elementary Course* (Toronto: The MacMillan Company of Canada, Ltd., 1924).

FIG. 21. INTERMITTENT CONTROL.
Surgeon and roentgenologist working simultaneously.

Figure 7: Here, a roentgenologist wearing a fluoroscopic hood is assisting the surgeon in localizing a foreign body.[195]

In Britain, radiologists complained bitterly that "the whole of the organization of military x-ray work has been carried out without any consultation with prominent radiologists." [196] They praised the "foresight" of American authorities who recognized "the importance of expert medical men in controlling the radiographic work."[197] This was in contrast to their own situation in Britain where they lamented, that "It seems to be the opinion of those in authority that any person capable of switching on an electric current is

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[195] J.S. Shearer, "Localization of Foreign Bodies: The Standard Methods Approved by the Surgeon General's Office, U.S. Army " *American Journal of Roentgenology* 5(1918): 245.

[196] G. Harrison Orton, "President's Address: The Necessity for Education in Radiology and Electrotherapeutics," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 11(1917): 1.

[197] Ibid.

quite well qualified to carry out electrical treatment."[198] Of course this portrayal of the situation in the United States was far too optimistic. American roentgenologists were equally concerned about the qualifications of those trained to operate x-ray installations.

The war put pressure on the American military establishment, which, like the British military, was obliged to employ many x-ray assistants with little training. Concerned doctors worried about the possible dangers to the soldiers being x-rayed:

> It should be constantly borne in mind that in the roentgen rays we deal with a potent means capable of doing great damage when unskillfully employed … the fact that sterility can be brought about by their agency is sufficient in itself to warrant the passage of laws preventing their use except by qualified persons.[199]

By 1917, training programs were in place in the United States to make sure that as much as possible those operating x-ray equipment in the military were "fully qualified medical men" who took "a real interest in this specialty and in this kind of work." [200] These men were given 3 months of instruction and 1 month of practical training, with the understanding that they "will not become in this short interval perfectly and fully qualified roentgenologists …[but] will be quite able to undertake practical work…"[201] Over the course of their instruction, medical corps students attended a short 14 week course on

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[198] Ibid., 2.

[199] "The Practice of Roentgenology in Laymen," *The American Journal of Roentgenology* 5(1918): 203.

[200] Rene Ledoux-Lebard, "On the Instruction of War Roentgenologists," *The American Journal of Roentgenology* 5(1918): 452.

[201] Ibid., 453.

roentgenology with 1 hour a week of lecture and 1 hour of practical lab instruction. The hope was, that this would at least, "give them a healthy respect for this specialty …"[202] Besides these rather hastily trained military doctors, there were technicians and 'manipulators' who received the same length of training but whose training was much more technical. The technicians took care of the apparatus and did repairs, while the manipulators made exposures of patients, developed plates and "work[ed] under the immediate direction of the roentgenologist." It was stressed that these assistants were not "meant to interpret roentgenograms or give opinions on plates they have taken, or to do fluoroscopy... It is not expected that they shall in any way usurp the duties of the roentgenologist."[203] The defining feature of the roentgenologist was not his ability to operate or repair the machinery or even make an x-ray plate, but to interpret the plate. Sound medical knowledge was as crucially important as knowledge of the equipment and the process used to take the picture.

The attitude that technical knowledge was necessary but by no means sufficient was often repeated by roentgenologists and radiologists defining a role for themselves that was separate both from their technicians and from other doctors. The American physician George Dock emphasized both the technical and medical knowledge necessary for roentgenology:

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[202] Howard Ashbury, "Annual Report of Roentgen Ray Department Army Medical School, Washington, D.C., 1917-1918," *American Journal of Roentgenology* 5(1918): 521.

[203] Ibid., 523.

> If the work is of a kind requiring not only skill, but also complex, costly and rapidly changing apparatus, it cannot be taken over generally as can simpler methods … The work then forms not a medical specialty, like ophthalmology, or internal medicine, but a technical specialty. If the one who cultivates it has little or no medical training he remains a technician unless by a combination of mental gifts, opportunity and hard work he acquires the other necessary knowledge.[204]

Besides the immediate goal of creating safe and efficient x-ray installations for the front, American and British x-ray physicians worried for the future of the profession and the maintenance of professional standards. The American doctor John Hall-Edwards made the familiar argument that in military settings,

> Every roentgen department should be supervised by a trained medical officer – under no circumstances should a layman be allowed to sign a roentgenological report. Nothing is likely to be more detrimental to the future of our science than the effects which are certain to accrue if this injunction be disregarded.[205]

But he went on to argue that these medical assistants should be warned that they would only be able to be assistants after the war. He was deeply concerned that these men would attempt to make money as consulting roentgenologists when the war was over.

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204 George Dock, "X-Ray Work from the Viewpoint of an Internist " *American Journal of Roentgenology* 8(1921): 321.

205 John Hall-Edwards, "Roentgenology in Warfare," *American Journal of Roentgenology* 5(1918): 281.

The first formal postgraduate programs in roentgenology began to appear in the United States following WWI, helping to institute professional standards of education in order to ensure that doctors supervising x-ray work had adequate training. Undergraduate medical students were to be taught how to interpret x-ray evidence, but "technique and original diagnostic work should be taken up by a course of apprenticeship later on …[since] not everyone as an undergraduate can learn how to use x-rays ..."[206] Beginning in 1917, all students at the Harvard Medical School received at least one month of instruction in roentgenology. These short courses were comprised mostly of clinical work with some lectures, and were not intended to train specialists. It was felt that "Every physician should also have sufficient knowledge of radiology to enable him to judge the work of the specialists in this field, so that he may select competent men to whom to refer his patients."[207] Specialists wanting to do their own diagnostic work could take half year courses while those hoping to specialize in roentgenology enrolled in a year long program with courses in physics, pathology and anatomy. After this coursework, students were to apprentice with a practicing roentgenologist for 5 years before setting up their own practice. Individuals encouraged to become roentgenologists were to be "selected from the student body, preferably those who show a special trend and adaptability, and who have had preparatory work in physics."[208] By 1920 several universities in the United States, besides Harvard, had established graduate programs in roentgenology. The Mayo Clinic at

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[206] Dock, "X-Ray Work from the Viewpoint of an Internist " 327.

[207] "Graduate Instruction in Radiology," *American Journal of Roentgenology* 9(1922): 465.

[208] Ibid.

the University of Minnesota announced a 3 year Masters program in Roentgenology in 1920.[209]

In Britain, postgraduate training in radiology was centralized in London. In 1917, a group of British radiologists formed a new committee, independent of both the Röntgen Society and the Royal Society of Medicine, whose key goal was the elevation of the professional status of radiologists and electrotherapists. Sir James MacKenzie Davidson was nominated chairman, and the first meeting was held at his house on April 4, 1917. This committee turned into the British Association of Radiology and Physiotherapy (BARP), and one of their primary aims was to establish a program for training radiologists. The first president of this new body explained,

> At present no standard exists: there is no organized teaching of the subject and those medical men who have taken up this branch of medicine have had to pick up their knowledge almost entirely by experience – at the expense of their patients.[210]

The group was successful in their efforts and in 1919, the Cambridge Senate approved the new Cambridge Diploma in Radiology. Hopes of professional uplift rested with this new formal program:

> The provision of a complete course of instruction and a Diploma, will aid, to an incalculable degree, the advance of a specialty which has been much neglected by

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[209] Louis B Wilson, "Graduate Education in Roentgenology," *American Journal of Roentgenology* 7(1920): 581.

[210] Orton, "President's Address: The Necessity for Education in Radiology and Electrotherapeutics," 3.

> the medical profession, and will help it to attain the position which it should rightly occupy.[211]

The censure by the medical community of unlicensed, commercial outfits selling electric treatments had tainted the professional reputation of electrotherapists, and x-ray doctors by association. These doctors hoped that formal education requirements would restore their image. Praising the new Cambridge Diploma, Dr. Barclay asserted, "To-day the man who fails to know the A, B, C of the science in which he deals is nothing less than a quack, and presumably a lazy one, for the knowledge is ready to hand to any man who cares to learn."[212] Rutherford took a similar tone noting, "We all hope that the day is past when anyone can pretend to give x-ray, radium or electrical treatment with little knowledge of the effects of radiation, and still less of the imposing apparatus he employs to impress the simple-minded patient."[213]

Candidates for the diploma were to take a two-part exam, with half of the questions on physics and half on radiology and electrology. In Rutherford's opinion,

> The expert in radiology must not only study the medical aspect of the question, but must have an expert knowledge of the working of his apparatus and a general acquaintance with the contributions of physics to the knowledge of the radiation which he employs.[214]

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211 "The Diploma in Medical Radiology and Electrology," 165.

212 Barclay, "President's Address: Ideals in Radiology and Electrology," 4.

213 Rutherford, "The Development of Radiology," 150.

214 Ibid.

The first course was held in London from February to July of 1920. The physics portion of the course was taught at University College London by L.H. Clark, under the direction of Sir William Bragg. 22 candidates took the examination in 1920, including medical graduates of the University of London, Edinburgh, Glasgow, Cambridge, Amsterdam, Melbourne and Adelaide. In order to obtain the diploma, each physician had to pass both sections of the exam. The first section, on Physics and Electrotechnics was comprised of two papers and a practical examination judged by Rutherford, JA Crowther and Sidney Russ. Section 2, on Radiology and Electrology also had two papers and an oral exam. A report on this first exam highlighted the nervousness surrounding the physics section in particular:

> The paper on physics was awaited with interest by all, and with anxiety by the examinees. In the opinion of the majority, the questions were perfectly fair. No question on higher mathematics was asked, and a knowledge only of simple calculations would be required to answer the questions in which a mathematical calculation was asked.[215]

Despite this anxiety, and perhaps indicating the technical inclinations of those who were drawn to this specialty, it was not the physics section of the exam that proved to be the most difficult: 17 people passed Part 1 but only 14 were successful in passing both parts (including one woman, Eva Muriel White).[216]

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[215] "The Diploma in Medical Radiology and Electrology."

[216] Ibid.

The Cambridge Diploma and the first American graduate programs in radiology shared an emphasis on the importance of physics for radiologists, but always with the admonition that knowledge of physics was crucial but not sufficient for creating a good radiologist. The American physicist J. S. Shearer was under no illusions that a physicist could be a roentgenologist. He defined a roentgenologist as “a medical man who is skilled in the use of roentgen-ray methods for the diagnosis or treatment of disease.”[217] Shearer argued: “The object is not to develop technical physicists or engineers, but to give a rational basis for instruction in the *use* of the roentgen ray apparatus.”[218] Furthermore, “Transplanting a physicist into a roentgen-ray laboratory will not lead to immediate results, and is quite as ridiculous as to put a physician in charge of a technical laboratory.”[219] Shearer felt that instruction in physics, “should be given by men well grounded in physics who have acquired an interest in medical problems and who can teach the subject without thinking the doctor should be a professional engineer.”[220] If we look again at Tables 1 and 2, it is clear that many physicists did indeed take up this post.

These physicists, however, tended to take an apologetic tone when speaking of the necessity of physics to the student of radiology. George Kaye captured this feeling when he said,

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[217] J.S. Shearer, "Graduate Instruction in Roentgenology," *The American Journal of Roentgenology* 9(1922): 459.

[218] Ibid.

[219] Ibid., 460.

[220] Ibid.

> I believe I can safely assume that the appreciation of the physicist by the medical worker is quite a new thing, for the average student, with one eye for forthcoming examinations, is prone to dislike and mistrust physics, and is never at ease until he is free of the subject.[221]

He wryly noted that courses on physics: "have not been received with any greater enthusiasm than that which medical students are wont to display towards anything savouring of physics."[222]

So why, then, force physics upon these students at all? In many ways, it did not need explicit justification. References to the foundational nature of physics to x-ray work were scattered throughout the literature on x-rays in this period. In a Kny-Sheerer catalogue from 1905, doctors were told that "The Roentgen technique is a technique which belongs to the domain of physics, and must refer back to that foundation at every step in its progress."[223] Many doctors appeared to have taken that foundation for granted. The first sentence of a 1912 paper on radiographic examinations of lungs affected by pulmonary TB read, "The laws of physics render it essential that the lungs shall follow the thoracic walls and the diaphragm in their movements."[224] This particular doctor did not feel that it was necessary to explain *which* laws of physics he was referring to in particular: these laws receded to reassuring if blurry shapes the background.

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[221] Kaye, "Radiology and Physics," 33.

[222] Robertson, *X-Rays and X-Ray Apparatus: An Elementary Course*: vii.

[223] Kny-Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*: 6.

[224] Charles Lester Leonard, "Compensating Displacements of the Thoracis Visera in Pulmonary Tuberculosis," *The American Quarterly of Roentgenology* 3(1912): 32.

Others invoked more immediately pragmatic reasons for requiring students of radiology to pursue physics beyond what they had encountered in their pre-medical or medical training. Many argued that a knowledge of the laws of physics would allow radiologists to understand how their apparatus worked. Rutherford praised the founders of the Cambridge diploma for insisting on the importance of adequate instruction,

> in the physical principles involved in the apparatus and radiations employed. In this way we may hope that the radiologist of the future will have sufficient knowledge of physics to understand fully the working of his apparatus and the character of the agents he employs in his work.[225]

Dr. Barclay echoed these sentiments, hoping not only that knowledge of physics would increase the efficiency of x-ray work, but would also stimulate interest in the work being done.[226] The American physicist J.S. Shearer argued that a knowledge of physics was crucial for any x-ray research. In his view, graduates in roentgenology should acquire: “Such familiarity with the physical laws underlying the subject as will protect him from publishing irrational papers or accepting them from others.”[227]

And yet, manufacturers routinely noted how easy it was to use their machines: In 1915, the Victor Electric Company advertised the simplicity of their apparatus,

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[225] Rutherford, "The Development of Radiology," 151.

[226] Barclay, "President's Address: Ideals in Radiology and Electrology," 5.

[227] Shearer, "Graduate Instruction in Roentgenology," 46.

> No expert mechanical knowledge is required for the successful manipulation of this apparatus. It is a fact that even an inexperienced operator can, within one hour, become sufficiently proficient in the manipulation to turn out good plates.[228]

In another catalogue, Victor reduced the training time even further noting,

> very few switches are used in manipulating the outfit to obtain any of the modalities or currents. We venture to say that the manipulation is so simplified that any intelligent operator can, by following the directions, become an expert in the manipulation of this apparatus within fifteen minutes.[229]

If the apparatus was really so simple to operate, then why bother with physics at all? It seems likely that these kinds of claims were vastly exaggerated for the purposes of advertising. Yet, it was the case that more and more the doctors weren't doing their own x-ray work; they were relying on technicians. And it was this relationship that furnished a second reason for learning physics. In order to have the moral authority to supervise others, doctors emphasized that the radiologist must know everything the technician knows (and more). The British radiologist Barclay argued:

> Of course it is essential that a man taking up this line of work should know his physics and electricity … He will have to be responsible for the work of his department and no man is fit to do this who does not know the general principles, and the detail, of every job that is carried out for him by assistants. This implies that he should also know the construction of his apparatus and the theory on which it is

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[228] Victor Electric Company, "Interrupterless X-Ray Transformers," (19151915).

[229] ———, *High Frequency Apparatus* (Chicago: Victor Electric Company, 1915), 3.

> constructed, for it is for him to say what is, and what is not, possible with the apparatus at his disposal. In short, *he must be the master* and must be able to speak with authority to his radiographer, photographer, or any other assistant, and to correct faults in their technique. [emphasis added][230]

This was furnished as a primary justification for physics instruction in the American community as well. In his defense of physics, Shearer argued: “Surely a well-trained roentgenologist should know his tools better than do those in his employ, otherwise he cannot secure the desired results and is too often at the mercy of the technician.”[231]

Besides allowing doctors to properly supervise technicians, a knowledge of physics also promised to provide protection against potentially dishonest manufacturers and distributers of x-ray equipment. Shearer noted that most doctors learned how to use apparatus from a salesman who, “talks glibly of things up-to-date, no matter how untried or ridiculous. He also by training is neither physician, scientist nor engineer, but he insidiously assumes the role of all three.”[232] Shearer felt that “Basic knowledge of the underlying principles, apparatus and measurements will tend to develop good sense and judgment in the purchase, arrangement and use of equipment. It should also tend toward emancipation from the dictation of agents, technicians and ill-informed people of all sorts whose influence is so unfortunate.”[233] In fact, many of the manufacturers themselves made

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[230] Barclay, "President's Address: Ideals in Radiology and Electrology," 3.

[231] Shearer, "Graduate Instruction in Roentgenology," 460.

[232] Ibid., 462.

[233] Ibid., 460.

the same kind of argument, hoping to position themselves as honest and trustworthy. These companies emphasized that they were on the doctors' side, against the unnamed companies who would take advantage of their ignorance. A Kny-Sheerer Company catalogue lamented that the first doctors "were novices who had ordered the building of Roentgen apparatus, novices who were not in a position to pass critical judgment upon the machines which they had ordered."[234] Doctors were urged to educate themselves. The R.V. Wagner Company told doctors that "Faith will become a small factor when selecting instruments best adapted to requirements … The buyer will be enabled to use his own judgment as governed by laws as reliable and well founded as Ohm's law."[235]

These companies stressed the importance of a thorough understanding of physical principles, urging doctors to learn everything they could about the construction and operation of their apparatus. But ultimately, they asked those same doctors to place their trust in their engineers, the invisible experts who ensured that their equipment was of the highest quality. Kny-Sheerer assured doctors that all of their apparatus "is made in our own factory by skilled mechanics and under the supervision of thoroughly competent electrical engineers."[236] The Victor Electric Company took a similar stance. Since, "nothing short of a thorough technical education would enable [the doctor] to determine as to whether an

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[234] Kny Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*: 6.

[235] R.V. Wagner Company, *Catalogue of Electrical Instruments for Physicians and Surgeons*, 7th ed. (Chicago: R.V. Wagner Co. , 1905), 3.

[236] Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*: 11.

electrical apparatus would properly perform its functions,"[237] the physician must trust that the engineers at Victor, "have full comprehension of our responsibility and our patrons may be assured that the designers of Victor apparatus have a thorough knowledge of the electrical, mechanical, and medical problems involved."[238]

Some knowledge of physics was deemed necessary for the practicing radiologist, but he or she would never be conversant enough to pass for a physicist or an engineer. Their extra training in physics was just enough to set radiologists apart from other doctors, and became a defining (though not always loved) feature of their professional identity. It was not clear to the first generation of x-ray doctors that physicists should be members of their professional societies, but the new calls for formal programs in radiology following WWI created a stable space for physicists as teachers. Students were told that this instruction was necessary, for their efficient and rational use of their equipment, and in order to properly supervise their technicians, and these students came to accept that physics was foundational to their practice.. In the next two chapters, we will see the impact of this increasing deference to the authority of physics on debates surrounding the best way to measure x-rays.

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[237] Victor Electric Company, *Catalogue No. 32 of Victor Electro-Surgical Apparatus* (Chicago: Victor Electric Company, n.d), 1; Barclay, "President's Address: Ideals in Radiology and Electrology.".

[238] Company, *Catalogue No. 32 of Victor Electro-Surgical Apparatus*: 1.

3. “A Surgeon without a Knife”

A New Era—X-ray therapy as Electrotherapy—Recalcitrant Rays—Measuring Devices—Standards Committees

3.1 A New Era

The discovery of x-rays gave doctors a startling glimpse into previously hidden parts of the body. But in addition to providing a novel tool for diagnosis, the new rays excited doctors with their therapeutic promise. In one of the first accounts of the physiological effects of x-rays, an operator at a public x-ray exhibition wrote to *Nature* in 1896 to document the painful inflammation and blisters he had suffered on his hands within weeks of beginning his x-ray demonstrations. After detailing the discomfort and pain he had endured for the past few months, he ended his letter with the optimistic hope that the rays might nonetheless prove useful in treating skin diseases in the near future.[239] Within a few years, this optimism was widespread among many medical practitioners. In his 1903 textbook, Dr. William Snow of New York promised his readers that, “a new era [was] dawning, and that the future [had] greater possibilities in store for the alleviation of human suffering.”[240] X-rays, applied both internally and externally, were especially exciting because they appeared able to cure diseases that had previously proved intractable, including many types of cancerous tumours and a number of skin diseases [Figures 8, 9,

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[239] S.J.R, "Some Effects of the X-rays on the Hands," *Nature* 54, no. 1409 (1896): 621.

[240] William Bentham Snow, *A Manual of Electro-static Modes of Application, Therapeutics, Radiography and Radiotherapy*, Second ed. (New York: A. L. Chatterton & Co. , 1903), 217.

10]. The enthusiasm for this new treatment was palpable in clinical accounts from the turn of the century, with doctors writing in to declare x-rays a permanent cure for a number of chronic conditions. Figure 11 shows a young girl treated for eczema by x-rays, her doctor boldly stating that with this new form of treatment, "no case of eczema need be regarded as 'hopeless'."[241]

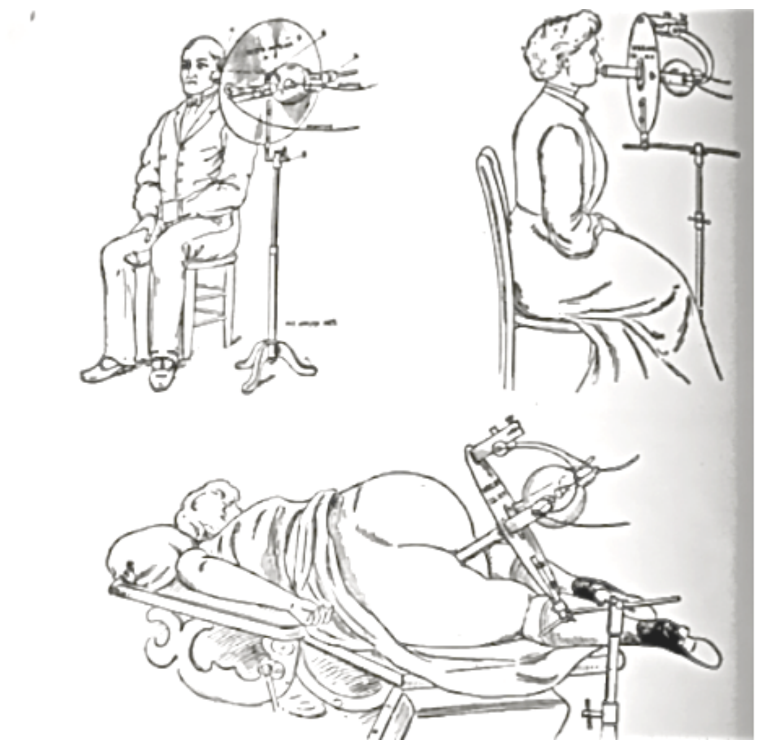

Figure 8: Advertisement for the Allen Shield, which was used to direct x-rays on to affected parts and protect surrounding surfaces.[242]

[241] Harold E. Gamlen, "The Treatment of Some Skin Diseases by X-Rays," *Archives of the Roentgen Ray and Allied Phenomena* 4(1904): 73.

[242] Waite & Bartlett Manufacturing Company, *Illustrated Catalogue and Price List*, 21st ed. (New York: Waite & Bartlett, 1905), 11.

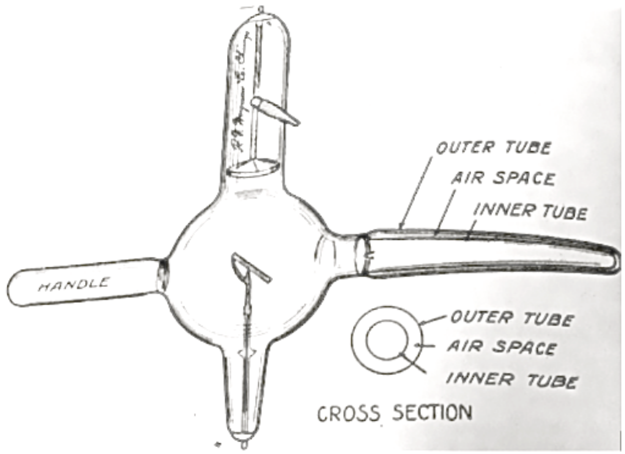

Figure 9: Cavity Treating Tube (1905).[243]

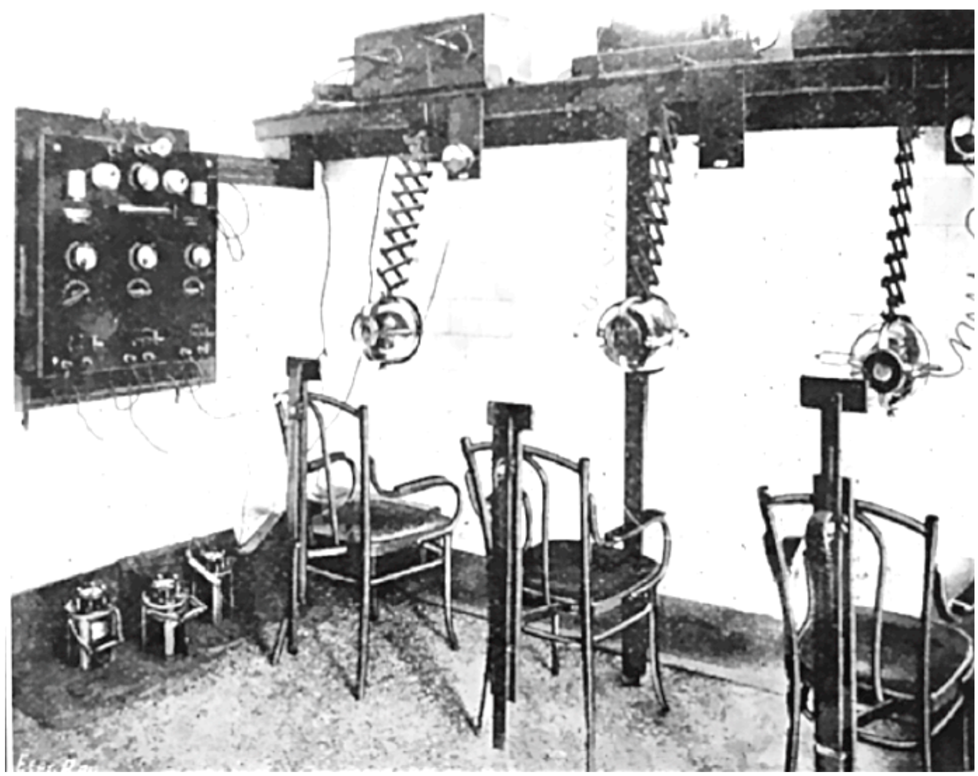

Figure 10: X-ray therapy for ringworm at the London Hospital (1905).[244]

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243 R. V. Wagner Company, *Catalogue of Electrical Instruments for Physicians and Surgeons*: 58.

Case 1: Before Treatment. Case 1: After Treatment.

Figure 11: Treatment of eczema by x-rays (1903).[245]

But it was evident that the same rays that could shrink a tumour or clear up a skin condition could also create unwanted burns, hair loss, and even new cancers. This new therapeutic agent was dangerous and unpredictable. Hundreds of the first x-ray workers died from overexposure to the radiation and were memorialized as martyrs to science.[246] And there were a number of sensational cases reported in newspapers throughout the early twentieth century of patients burned, sometimes horrifically, by x-rays. In one particularly harrowing case in 1923, a coroner who had been called in to investigate a patient death concluded that the individual had committed suicide “while of unsound mind” caused by intense agony due to an x-ray burn on his back. The deceased had recently been x-rayed at Victoria Hospital in London, and the doctors couldn’t figure out what had

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[244] James H. Sequeira and Reginald Morton, "The Light, X-Ray and Electrical Departments at the London Hospital," *Archives of the Roentgen Ray and Allied Phenomena* 10(1905): 270-74.

[245] Gamlen, "The Treatment of Some Skin Diseases by X-Rays," 72-75.

[246] Herzig, *Suffering for Science*. These themes will be explored in more depth in Chapter 5.

gone wrong; the apparatus appeared to have been working normally.[247] In another case, at a Baltimore hospital, doctors were baffled by a series of incidents of severe side effects from x-ray treatment. Most of the patients receiving x-ray therapy in the hospital reacted as expected, but a handful of others reported horrible burns, and one child even suffered permanent hair loss. The doctors couldn't discern a pattern until it was discovered that the x-ray apparatus was running off of the same electrical circuit as the hospital elevator. The dose delivered by the x-ray tube had been measured initially when the elevator was running. When the elevator stopped, the current to the x-ray room doubled, and the patients received a much higher dose of radiation.[248]

The Baltimore case shows just how difficult x-rays were to monitor. They were invisible and they were insensible. The burns and other side effects that might indicate overexposure often took weeks to develop. This meant that the problem of discovering a consistent and reliable method of measuring the dose of x-rays delivered to the patient was of utmost importance to early radiologists. And it was a problem that occupied the interest of physicists as well. But the communities of physicists and doctors involved in x-ray work had very different visions of how best to measure x-rays for therapeutic purposes. The debates in this chapter and the next over what constituted an appropriate measurement of a dose of x-rays clearly illuminate the divergent values of these two cultures. The ideal of medicine as an art, the unquantifiable skill of the physician, and a 'good enough' pragmatism clashed with physicists' emphasis on precision and objectivity.

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[247] "Burns from X-Rays: Suicide of Injured Patient," *The Times* September 12 1923, 8.

[248] George M. MacKee, "Arithmetical Computation of Roentgen Dosage," *American Journal of Roentgenology* 6(1919): 607.

In this chapter, I will show that this clash of cultures was evident even before the discovery of x-rays, rooted in the earlier practices of 19th century medical electricity. I will then give an overview of available methods of x-ray measurement in the early 1900s and show the growing disagreement between physicists and doctors as committees were formed in both the United States and Britain to establish a unit of radiation measurement. In the next chapter, I will follow these debates into the 1920s, when the röntgen was officially adopted as the international unit of x-ray measurement, highlighting the increasing authority of physicists in both the American and British x-ray communities. By 1928, physicists had taken on a strong leadership role in radiology, successful in their calls for a new method of measurement despite any clear indication from practicing doctors that this new method was in fact necessary, or even desirable.

3.2 X-ray Therapy As Electrotherapy

When x-rays entered medical practice, doctors who had been practicing electrotherapy found that they were already comfortable with the technology needed to produce x-rays. The same equipment used to apply different modes of electricity to the body could also be used to power x-rays bulbs, and radiotherapy was often incorporated into existing electrical departments. The out-patient electrical department established at the London Hospital in 1903, for instance, included rooms for treatments by high frequency electricity, electric baths and finsen light as well as treatment by x-rays and radium for skin

diseases and tumours.[249] And just as x-ray technology joined a debate already in progress concerning the proper place of instruments in medical diagnosis, the use of therapeutic x-rays raised a number of concerns already familiar to those practicing electrotherapy. Questions concerning the applicability of the laws of physics to medicine, the establishment of consistent dosage and the quantification of the human body were familiar to the late 19th century electrotherapists who were often among the first to experiment with x-rays.

Iwan Morus has argued that this small but increasingly professionalized community turned to the world of electrical science for legitimacy. Besides sharing the same technology, Morus contends that "the medical electrician's culture shared much … with that of the professional electrical engineer."[250] This cultural overlap was not, however, wholly without tension. In particular, the commercial ethos of engineering proved to be a liability as electrotherapists in both the United States and Britain sought to increase their status within the medical community. Commercial electrotherapy institutes and the sale of equipment marketed for home use transgressed the professional ethics of late 19th century medicine. Doctors who were affiliated with these endeavours faced censure from their colleagues.[251] On the other hand, by embracing the desire for precision measurement

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[249] The Electrical Department at the London Hosptial saw an estimated 20 000 patients in 1904. Sequeira and Morton, "The Light, X-Ray and Electrical Departments at the London Hospital," 270-74. Other British hospital electrical departments are described in detail in Burrows, *Pioneers and Early Years*. See also Guy, "The development of Radiology in Britain 1896-1921 and factors influencing its growth."

[250] Iwan Rhys Morus, "The Measure of Man: Technologizing the Victorian Body," *History of Science* 37(1999): 260-61.

[251] Takahiro Ueyama, "Capital, Profession and Medical Technology: The Electro-Therapeutic Institutes and the Royal College of Physicians," *Medical History* 41(1997): 150-81.; Iwan Rhys Morus, "Batteries, Bodies and Belts: Making Careers in Victorian Medical Electricity," in *Electric*

valued by physicists and engineers, electrotherapists hoped to dress their practice in a mantle of authority borrowed from physics.[252] But even while many of these doctors proclaimed that physics knowledge was essential to the rational application of electricity to the body, the limits of this knowledge were beginning to be apparent. Those advocating a model of the body as a quantifiable electrical machine clashed with others who emphasized the idiosyncratic reactions of different bodies to the action of electricity. When x-rays joined the arsenal of the electrotherapist, these debates became even more pronounced.

The practice of applying electricity in medicine dates back to at least the 18th century. In their sweeping history of electricity and medicine, Charles Rowbottom and Margaret Susskind credit Johann Gottlab Krüger, a professor of philosophy and medicine at the Lutheran university of Helmstäst, as the first European physician to consider the medical use of electricity in 1743.[253] Those who marketed electrotherapy to the public often appealed to ancient Greek examples of the use of electric eels to establish an even longer continuous practice. But as Paola Bertucci and Giuliano Pancaldi have argued, despite this long history, electrotherapy was never able to coalesce around a coherent set

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Bodies: Episodes in the History of Medical Electricity, ed. Paola Bertucci and Giuliano Pancaldi (Bologna: Universita di Bologna, 2001), 209-38.

[252] ———, "Bodily Disciplines and Disciplined Bodies," *Social History of Medicine* 19(2006): 249. Historians have argued that the aesthetic of science was equally important in marketing this technology to the public. See for instance Lori Loeb, "Consumerism and Commerical Electrotherapy: The Medical Battery Company in Nineteenth Century London," *Journal of Victorian Culture* 4, no. 2 (1999).; Caroline Thomas de la Pena, *The body electric: How strange machines built the modern American* (New York New York University Press, 2003).

[253] Margaret Rowbottom and Charles Susskind, *Electricity and Medicine: History of their Interaction* (San Francisco: San Francisco Press, 1984), 7.

of practices or theories.[254] Practitioners throughout the 18th and 19th centuries disagreed over how best to apply electricity, over its mode of action on the body and when its application was even appropriate. It was well established by the late 19th century that local electrization could help stimulate paralysed muscles and electrolysis and electrocautery were increasingly used in surgery to break up tumours and stop bleeding. But in addition, its most enthusiastic proponents claimed that electricity was useful in a wide range of chronic conditions including insomnia, insanity and hysteria, as well as diseases of the skin, heart, lungs and digestive system, diabetes, whooping cough, tuburculosis and sexual disfunction.[255]

Electricity gained an institutional home in London in the electrifying room at Guy's Hospital in 1836 under the direction of Golding Bird. This room was unique in England until the 1860s, but by the 1870s and 80s electrical departments were beginning to be established in a number of general hospitals.[256] In the United States, the growth of the specialty followed a similar timeline, with electrotherapy gaining the greatest professional

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[254] Paola Bertucci and Giuliano Pancaldi, "Introduction," in *Electric Bodies: Episodes in the History of Medical Electricity*, ed. Paola Bertucci and Giuliano Pancaldi (Bologna: Universita di Bologna, 2001), 5-15.

[255] The most influential American textbook in the last quarter of the 19th century was George Beard and Alphonso Rockwell, *A Practical Treatise on the Medical and Surgical Uses of Electricity*, 3rd ed. (New York: William Wood and Company, 1881). For a representative British text see W.E. Steavenson, *Electricity and its manner of working in the treatment of disease* (London: J&A Churchill, 1884).

[256] Iwan Rhys Morus, *Frankenstein's Children: Electricity, Exhibition, and Experiment in Early Nineteenth Century London* (Princeton: Princeton University Press, 1998). See Chapter 8: "Under Medical Direction: The Regulation of Electrotherapy," p.232-255.

credibility in the 1880s and 90s with its own societies and journals.[257] Lisa Rosner has argued that the practice of electrotherapy in America peaked in the 1890s and links the subsequent decline in interest in electrical treatments to the discovery of x-rays: "the glamor, as well as the therapeutic and research opportunities of the X-rays, took much of the excitement away from [electricity]."[258]

But Rosner has overstated the disjunction between electrotherapy and radiotherapy in the first decades of the 20th century. The x-ray tube being used to treat cancer of the breast in Figure 12 is powered by a static electricity machine that could also provide electrical treatments. Figure 13 shows the same type of apparatus being used to dispense "static breeze" to patients in 1910 and Figure 14 shows the McIntosh Static and X-ray Machine. And it wasn't just these older static machines that were reappropriated for use with x-rays. New combination office and portable machines were designed to fulfil many functions, allowing practitioners to take x-ray pictures, give x-ray therapy and electrotherapy and even supply current for heating cautery knives and lighting diagnostic lamps, all with the same apparatus (Figures 15 and 16). Major suppliers of x-ray equipment in the early 20th century including Waite and Bartlett, R.V. Wagner Co., The Victor Electric Company, Thompson & Blaster Co. and the McIntosh Battery and Electrical Company advertised machines for both electrotherapy and x-ray therapy into the 1920s.[259] Clinical

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[257]Lisa Rosner, "The Professional Context of Electrotherapeutics," *Journal of the History of Medicine and Allied Sciences* 43(1988): 64-82.; Lawrence D. Longo, "Electrotherapy in gynecology: The American Experience," *Bulletin of the History of Medicine* 60(1986): 343-66.

[258] Rosner, "The Professional Context of Electrotherapeutics," 81.

[259] Waite & Bartlett Company, *Illustrated Catalogue and Price List*.; Company, *Catalogue of Electrical Instruments for Physicians and Surgeons*.; Victor Electric Company, *Roentgen Apparatus and High Frequency Outfits* (Chicago: Victor Electric Company, c.1910).; Thompson & Blaster Co. Inc.,

papers on electrotherapy appeared alongside papers on radiotherapy and radiography in medical journals throughout this period.

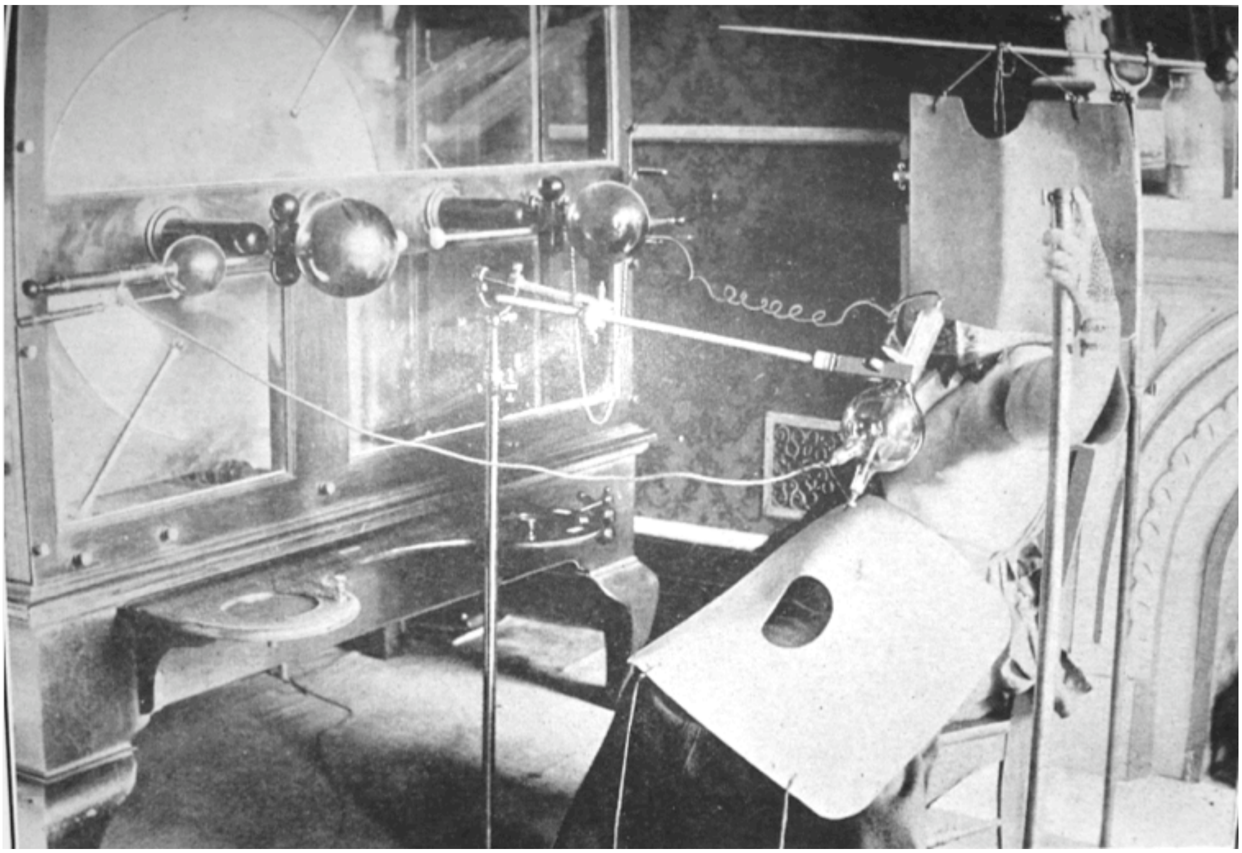

Figure 12: Treatment for cancer of the breast (1903). [260]

Directions for Operating Style 'E' Combination Physician's Electrical Cabinet (Leesburg, Va.: Thompson & Blaster Co. Inc. , c.1905).; McIntosh Battery and Electrical Company, *McIntosh Electro-Therapeutical Catalogue: Thirty-Second Edition* (Chicago: McIntosh Battery and Electrical Company, 1911).

[260] Snow, *A Manual of Electro-static Modes of Application, Therapeutics, Radiography and Radiotherapy*.

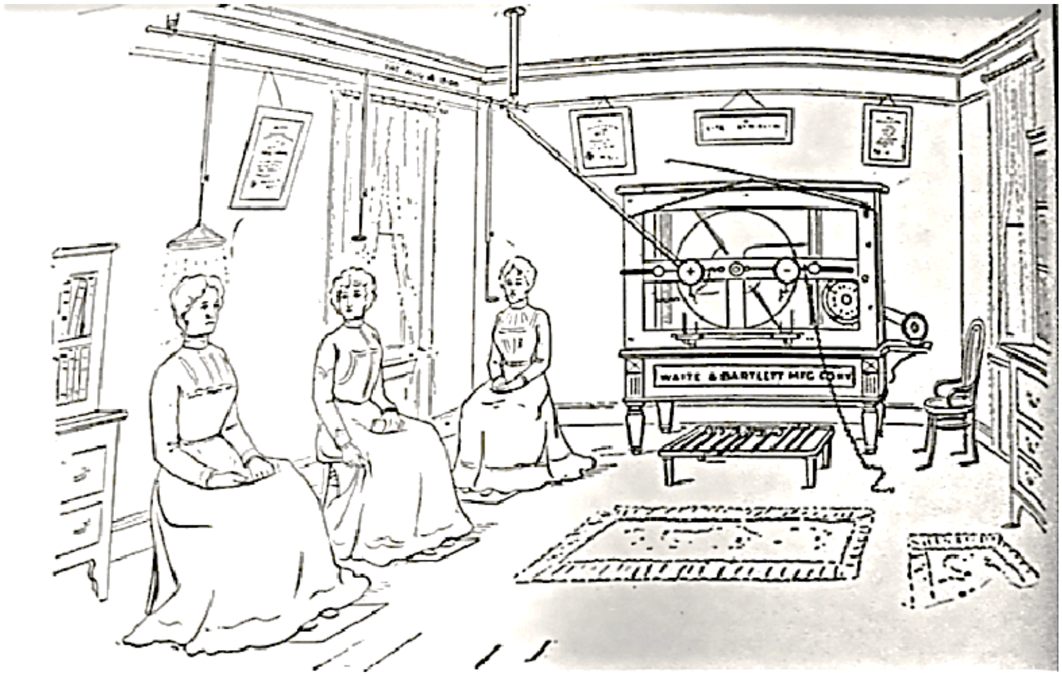

Figure 13: Patients receiving Static Breeze Treatment (1910).[261]

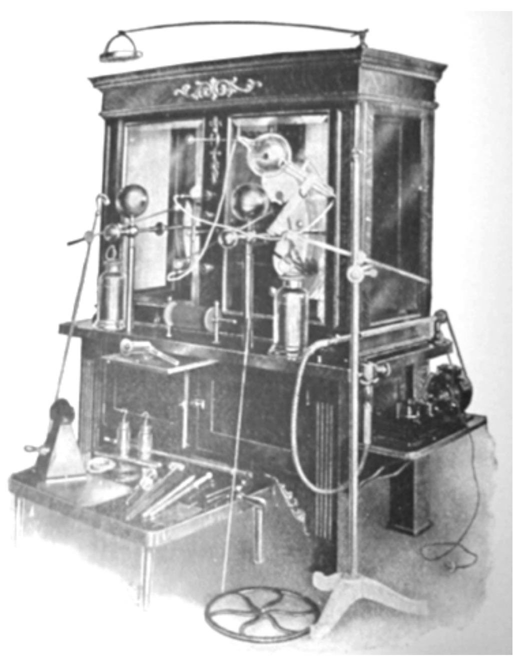

Figure 14: McIntosh Type "A" Model Static and X-Ray Machine (1911) [262]

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261 Waite & Bartlett Manufacturing Company, *X-Ray and High Frequency Apparatus*, 22nd ed. (New York: Waite & Bartlett, 1910), 52.

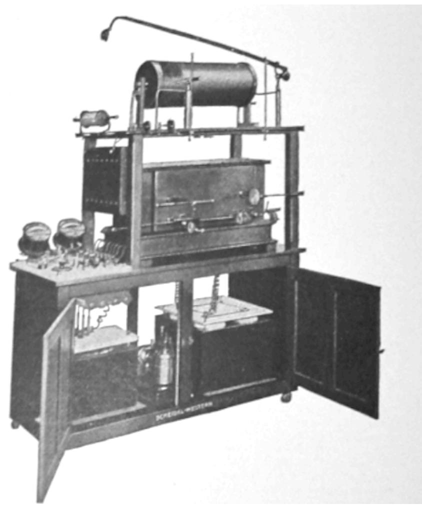

Figure 15: McIntosh Combination X-Ray and High Frequency Outfit (1911).[263]

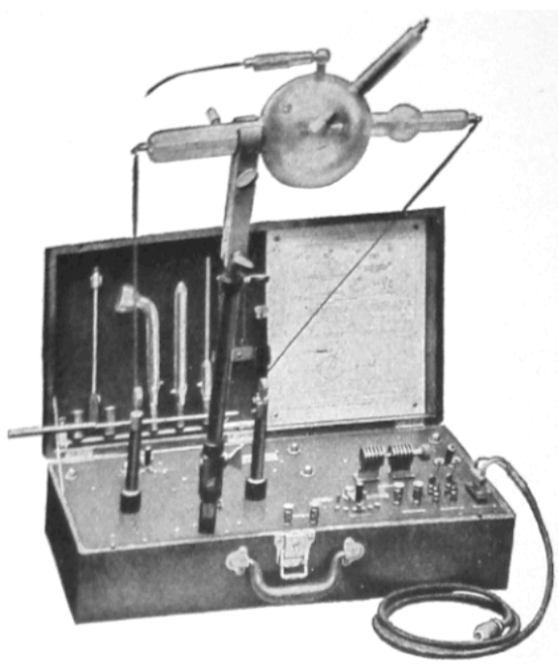

Figure 16: McIntosh Radiographic Suit Case Portable Coil.[264]

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[262] Company, *McIntosh Electro-Therapeutical Catalogue: Thirty-Second Edition*: 162.

[263] Ibid., 148.

Rather than redirecting interest away from electrotherapy, the discovery of x-rays was seen by radiologists to have increased interest in medical electricity overall. Speaking to the profession in 1920, A. E. Barclay declared, "the advent of X-rays, with their obvious utility in so many directions, was an enormous asset in the establishment of the value of the other uses of electricity."[265] The Royal Infirmary in Edinburgh, for instance, had had no separate electrical department or even room prior to the discovery of x-rays. By 1908, the electrical department consisted of 12 rooms, and each day staff in the department treated 10 cases by x-rays on average, in addition to seeing approximately 7 cases for diagnostic x-rays.[266]

The reliance of radiotherapeutic practice on complex instruments raised the same kinds of concerns as electrotherapeutic practice had decades earlier. Sophisticated technology seemed to displace the skill of the operator: if the battery was well made, then it seemed that anyone might use it successfully. In their influential textbook on electrotherapy, George Beard and Alphonso Rockwell deliberately set out to dispel the idea that all one need do to practice electrotherapy is turn on a current:

> A good battery is not all that is necessary to make a good electrotherapeutist. … The purchase of a battery is simply a first step in the right direction, it is the

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[264] Ibid., 160.

[265] Barclay, "President's Address: Ideals in Radiology and Electrology," 2.

[266] Dawson Turner, "Some Reflections based upon the work done in the Electrical Department of the Royal Infirmary, Edinburgh," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 1(1908): 118.

> beginning of a long road. One who uses electricity in medicine requires good apparatus, just as the surgeon requires good instruments and the carpenter good tools; but as tools cannot make a carpenter, nor instruments a surgeon, so a battery cannot make one skillful in the therapeutical use of electricity. It is not the battery, it is the brains, that makes a good electro- therapeutist.[267]

The same concern was evident decades later. In his presidential address to the American Roentgen Ray Society in 1910, George Pfahler lamented, "It will take considerable time until the profession even realize that x-ray instruments no more make a Roentgenologist than surgical instruments make a surgeon."[268] Popular reports of this new therapeutic modality contributed to this concern. In a piece in *The Times* in 1921 describing the dangers of x-ray therapy, x-rays, rather than the operator, were given agency: "The most remarkable property of the new agent is its power of penetrating living tissue and acting as a surgeon without a knife."[269] The x-rays themselves act as the surgeon. The radiotherapist is absent.

So what, then, set apart an electrotherapist (or a radiotherapist) from anyone who might turn on a current or power an x-ray bulb? For Beard and Rockwell,

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[267] Beard and Rockwell, *A Practical Treatise on the Medical and Surgical Uses of Electricity*: 291.

[268] George E. Pfahler, "Presidential Address: The Duty of the Public and the Roentgenologist," *American Quarterly of Roentgenology* 2(1910): 263.

[269] "The Danger of X-Ray Therapeutics," *The Times (London)*, March 30 1921, 9.

> The more thoroughly one studies electrotherapeutics … the clearer it becomes that the real scientific basis for the use of electricity in medicine and surgery is found in electrophysics more than in electrophysiology.[270]

These doctors argued that there was an urgent need for education in physics because the physics presented in electrotherapy textbooks tended to be far behind the time and what was expressed in physics texts was "expressed so blindly as to be of little value."[271] Their extensive textbook opens with a section on physics and it is worth briefly examining the role of physics in this book. Beard and Rockwell explicitly endorsed physics, yet clear lines of tension are evident in their work: the most up to date physics knowledge could not be applied to medicine unproblematically.

Beard and Rockwell began with an ontological discussion, explaining to their readers that electricity is a force, like heat and light, and like them is a mode of motion of the ether. Yet they went on to use the fluid model of electricity exclusively in the rest of the book, both to explain physics concepts and to account for medical observations. In order to explain the idea of electrical resistance, for example, and the relationship given by Ohm's law, they asked their readers to imagine a current of water passed through an ordinary syringe. The amount of water expelled, they explained, would depend on the initial force of the piston and the friction encountered by the water inside the tube, just as the quantity of electricity in a wire depended on the electromotive force of the source and resistance of the

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270 Beard and Rockwell, *A Practical Treatise on the Medical and Surgical Uses of Electricity*: vi.
271 Ibid., xv.

wire.[272] When they explained the ways in which patients varied in their susceptibility to electricity, readers were told that for certain people, "Electricity can be poured into them in limitless measures, they may be saturated with it."[273] For Beard and Rockwell, it was easiest to think of electricity as a fluid poured into the body, rather than a series of vibrations in the ether.

The fluid model was still, however, a model that would have been sanctioned by physicists, if only for pedagogical purposes.[274] There are other moments, though, in which Beard and Rockwell struggle to reconcile what physics tells them about electricity with their experiences applying electricity clinically. In particular, they know that, according to physics, there is only one type of electricity: that static electricity, current from a chemical cell and current produced by electromagnetic induction are all modes of motion of the ether. But this made it difficult for them to explain the different physiological actions of currents produced by different sources. One entire section of their text was devoted to an exploration of when it is best to use galvanic versus faradic currents (galvanic being the current produced by a chemical battery, and faradic being electromagnetically induced current). The galvanic current, they explained, has a stronger chemical action, it affects the

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[272] Ibid., 73.

[273] Ibid., 254.

[274] Fluid models of electricity were popular in the late 18th century, championed by natural philosophers such as Coulomb. By the time of this 1881 edition of Beard and Rockwell's treatise, these fluid models had been replaced in Britain by a field theory based on the work of Faraday and Maxwell in which electric effects were due to the state of the surrounding aether. In Germany, a leading theory of electrodynamics was that of Weber, who proposed that electric phenomena were due to the movement of electric atoms. See Jed Buchwald, *The Creation of Scientific Effects* (Chicago: University of Chicago Press, 1994).; Bruce J Hunt, *The Maxwellians* (Ithaca, NY: Cornell University Press, 1994).; Robert Purrington, *Physics in the Nineteenth Century* (London: Rutgers University Press, 1997).

brain and spinal cord more powerfully and produces muscle contractions where faradic fails. The galvanic produces a feeling of burning where the faradic feels like stinging and pricking. The authors attempted to explain this by arguing that the difference was one of degree, but even this didn't sit completely well: "And yet the difference in degree between the effects of the two currents is so marked and so clearly demonstrable as to be practically equivalent in certain instances to a difference in kind and to give very important and remarkable advantages to one current or the other."[275] In another attempt to explain the difference, Beard and Rockwell appealed to the fact that the galvanic current was steady while the faradic current had constant interruptions. But then later in the same section they discussed different faradic machines and their clinical use, arguing that certain types of faradic devices produced a current that was less irritating than others'. The difference between the current produced by different versions of faradic machines, all of which produce current by electromagnetic induction, could not be explained by any appeal to physics. Beard and Rockwell's readers would certainly have been left with a sense of the ontological plurality of electricity.

The section in which Beard and Rockwell were best able to demonstrate the practical value of physics was also the section in which they argued, ultimately, for the rejection of physics in favour of the clinical skill of the practitioner. This was the section in which they talked extensively about Ohm's Law, the relationship that states that the quantity of electricity passing through a wire is proportional to the electromagnetic force and inversely proportional to the resistance in the wire. They called this law, "the north

[275] Beard and Rockwell, *A Practical Treatise on the Medical and Surgical Uses of Electricity*: 263.

star of dynamical electricity. Those who can keep this always in sight need never lose their way …"[276] They were able to point to a number of practical considerations in medical practice that can be explained with reference to the law. They debunk, for instance, the popular idea that larger cells send more electricity through the body. Familiarity with Ohm's law, they argued, would show the reader that this assumption was incorrect. The same type of cell will have the same electromagnetic force and will produce the same quantity of electricity for a given resistance. But the most important result of Ohm's law was what it could tell a doctor about the possibility of being able to specify a particular dose of electricity. Imagine, they argued, that you are passing on a patient to a new physician and you wanted this doctor to continue to give the patient the same dose he or she had been receiving. You might tell this new doctor that you have been using ten cells for ten minutes. "In the light of Ohm's law," Beard and Rockwell asked, "let us see what such instructions are worth."[277] It turned out that they were not worth very much. The new doctor might not have the same equipment, and even if this new doctor used an identical battery, the emf of any battery will change over time. The internal resistance of the battery was similarly variable and could change while the battery was in operation. But the biggest problem was in the external resistance, which depended on the size and shape of the electrodes, whether they were doused in water or salt water or were dry, the amount of pressure used and the position and the extent of the parts of the body included between the electrodes. Impossible to ignore, as well, were the idiosyncrasies of the patients. According to Beard and Rockwell, Germans were more susceptible than the British to the

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[276] Ibid., 65.

[277] Ibid., 80.

action of electricity, the leisured classes were more susceptible than labourers and women more than men. All of this meant that a doctor could not simply prescribe the number of cells or the time of application. The authors held out hope that the "Newton of physiology" might come to make sense of the chaos, but until then, all one could do was specify mild, medium, or strong current and then specify the type of current, and, "we will have attained all the accuracy that science will allow."[278]

Ohm's law gave Beard and Rockwell the vocabulary to talk about all of the variables that could not in fact be quantified. In their words,

> No forms of error are so erroneous or so illusory as those that approach us under cover of facts and figures. In our very attempt to be accurate we stumble into gross inaccuracy. Had we left the whole matter to the judgment of the physician with some general suggestions as to the susceptibility of the patient, we should have come far nearer the truth.[279]

Christopher Lawrence has studied a particular community of British doctors at the turn of the twentieth century who were resistant towards new diagnostic technologies that seemed to threaten the clinical art, or the incommunicable knowledge of the physician.[280] But here, those welcoming these new technologies— doctors who were basing their practice on these technologies—were still appealing to a physicians' special judgment.

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[278] Ibid., 83.

[279] Ibid., 80.

[280] Lawrence, "Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain, 1850-1914," 503-20.

Batteries in the clinic might be governed by the rules of physics in principle, but not in practice. When x-rays appeared as the next modality of electric treatment, the direct applicability of physics to medical practice was similarly challenged, both by idiosyncratic bodies and resistant doctors.

3.3 Recalcitrant Rays

A snapshot of medical practice in 1910 reveals that while the diagnostic use of x-rays was well established, their therapeutic use was still not as widespread. A survey of the members of the American Roentgen Ray Society shows that while 80% of members were involved in diagnostic work, only 50% were involved in therapy. 30% used x-rays for both diagnostic and therapeutic purposes [Figure 17]. A survey of 58 hospitals with x-ray apparatus showed that while all of the hospitals were using x-rays for diagnostic purposes, 40/58, or 69% used this equipment for therapeutic purposes.[281] The situation in England was similar. At St. Thomas's Hospital, in the same period, ten times as much diagnostic as therapeutic work was being done. At this hospital, in 1913, 6501 radiographical examinations were performed, 609 cases were treated with x-rays and 500 cases were treated with electricity.[282] Because only a small number of physicians practiced x-ray therapy exclusively, a formal separation between therapy and diagnostics didn't take place

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[281] Rollin H. Stevens, "X-Ray Work in Hospitals," *American Quarterly of Roentgenology* 2(1910): 111.

[282] C. Gouldesbrough, "Summary of the Work in the X-ray Department at St. Thomas's Hospital for the Year 1913," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 7(1914): 25-28.

in radiology until the 1930s. In 1934, the Society of Radiotherapists was formed in Britain and the first separate postgraduate examination in radiotherapy was administered in 1937. Even at this point, however, many radiologists continued to practice both diagnosis and therapy.[283] In Canada, the Canadian Association of Radiologists was formed in 1937, and separate examinations in diagnosis and therapy were instituted in 1946.[284] In the United States, this separation happened later, with an American Club of Therapeutic Radiologists forming in 1958.[285]

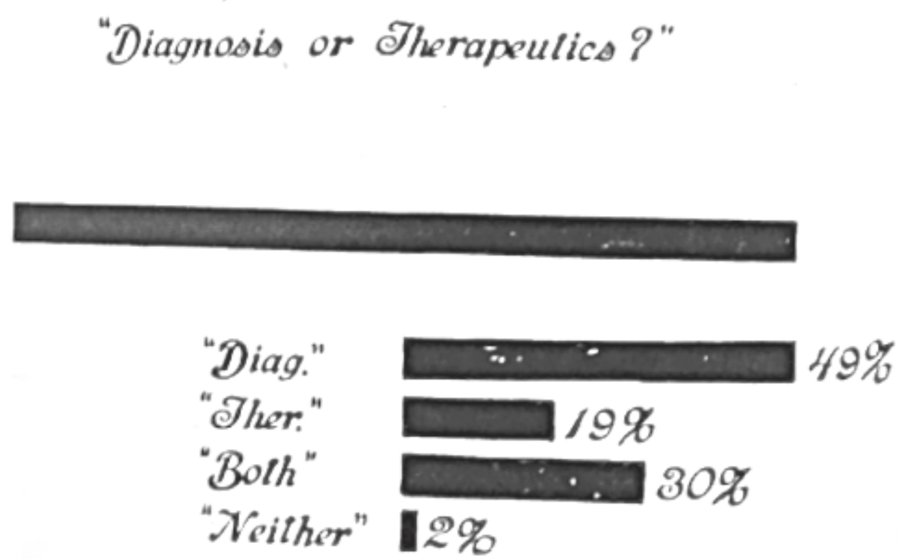

Figure 17: Results of a 1910 survey asking members of the American Roentgen Ray Society whether they were engaged in diagnostic or therapeutic x-ray work.[286]

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283 J. Michael Henk, "A Brief History of British Radiotherapy," *International Journal of Radiation Oncology* 36(1996): 213-18.

284 Hayter, "Radiation Oncology in Canada 1895-1995.".

285 Radiological Society of North Radiological Society of North America, "The Golden Era," http://www.rsna.org/Part_8_The_Golden_Era.aspx.

286 "Secretary's Report 1910," *American Quarterly of Roentgenology* 2(1910): 260.

Why was diagnostics established more quickly? Besides fighting to transcend their association with electrotherapy, a specialty forever teetering on the fringe of respectability, those interested in pursuing radiotherapy in the early 20th century would certainly have been deterred by problems of measurement. Doctors were motivated to determine consistent means of x-ray dosage by the horrible burns that could result from overexposure to the rays. An overdose of any drug in medicine could be similarly serious but x-rays presented a unique problem because they were particularly hard to measure. Medical therapies based on drugs could at least use some measure of the weight of a particular compound to specify a dose. Of course, even then, determining the appropriate medical dose proved much more problematic as evidenced by debates over the 'cat dose' used to measure digitalis in the 1910s. Defined as the minimum fatal dose per kilogram of cat, researchers argued over the variability of individual cats in their response and the suitability of different test animals.[287] But even leaving aside problems of physiology, there was no equivalent means of isolating a consistent amount of x-rays. X-rays were of course weightless, and to make matters even more frustrating, the early x-rays tubes were far from consistent in their output. In 1904, one user expressed excitement over a tube running steadily for 15 whole minutes.[288] Speaking in 1907, another British doctor expressed his frustration with the tubes saying: "To measure a Roentgen ray tube is very much like measuring a will o' wisp; it is one of the most freakish and capricious things

[287] Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1995), 30.

[288] W. Deane Butcher, "The Means of Accurate Measurement in X-Ray Work," *Journal of the Röntgen Society* 1(1904): 81.

which it is possible to deal with."[289] There were hundreds of tube designs on the market in the early years of the 20th century, but even two x-ray tubes of the same make couldn't be expected to give a similar output. The same tube wouldn't even perform consistently from day to day. Doctors learned to judge the vacuum of the tube from the colour of the discharge inside. Figure 18 shows diagrams from a radiology textbook instructing doctors how to recognize the appropriate level of vacuum from the colour of the discharge. If the vacuum was too high, it could result in sparking at the electrodes, and if too strong a current was turned on, the tube might be punctured. If the vacuum was too low then it might not produce x-rays at all.[290] Doctors dreamt of a future tube that would be "an ethereal musical instrument able to give out and sustain without alteration of pitch any required note on the Röntgen gamut."[291] But the physicists and engineers involved in x-ray work had to admit that it seemed too much to expect to be able to control all of the slight variations that occurred while an x-ray tube was in operation. Mr. Gardiner judged it "practically impossible" for manufacturers to standardize the tube.[292] Physicist George Kaye agreed, admitting that, "even if agreement in design were secured, the performance of a bulb is peculiarly susceptible to slight variations in the prevailing conditions, over some

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289 Lord Blythswood and Walter A. Scoble, "The Relation between measurements from a focus tube, with a view to determine which are proportional to the intensity of the Röntgen rays," *Journal of the Röntgen Society* 3(1907): 62.

290 Sinclair Tousey, *Medical Electricity, Röntgen Rays and Radium*, 2nd ed. (Philadelphia: W.B. Saunders Company, 1915), 783-85.

291 W. Deane Butcher, "The Future of Electricity in Medicine " *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 1(1908): 2.

292 ———, "The Means of Accurate Measurement in X-Ray Work," 78.

of which control is scarcely possible."[293] Rev. Ph. Mulholland felt that the whole endeavour of standardization might be impossible:

> The medical practitioner knows already how many things which come within the scope of his ordinary work vary very greatly, but over and above these we learn that the tube itself varies in many ways, and also affects the final result. In view of all these variable elements it appears almost hopeless to attempt the work of comparison.[294]

There was a broad consensus, at least prior to the invention of the Coolidge tube, that standardization of x-ray dosage would not be achieved by standardizing the equipment. There had to be a way to monitor the properties of the x-rays as they were produced at any given moment.

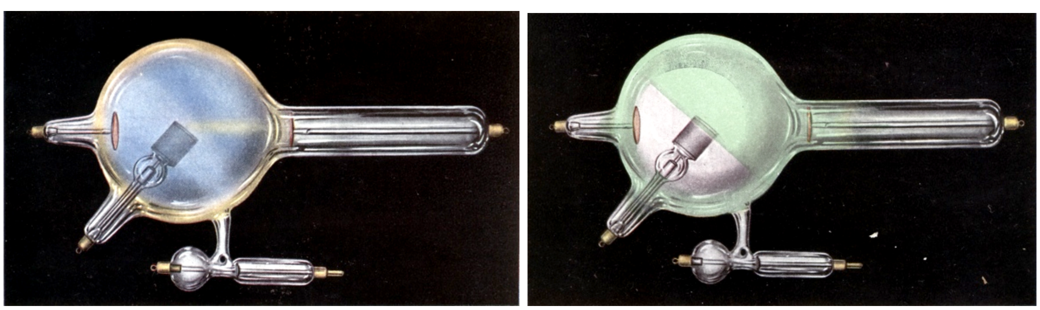

Figure 18: Gas X-ray tubes. The one on the left shows too low of a vacuum, the one on the right shows a good level of vacuum.[295]

[293] Kaye, *X-Rays*: 89.

[294] Blythswood and Scoble, "The Relation between measurements from a focus tube, with a view to determine which are proportional to the intensity of the Röntgen rays," 63..

[295] Tousey, *Medical Electricity, Röntgen Rays and Radium*., plate 13 and plate 10.

The physics community did not reach a clear consensus as to the physical nature of the x-rays until almost 20 years after their discovery,[296] but those who worked with the rays understood right away that there were two important properties that had to be measured. The first was the quantity of x-rays hitting a particular target and the second was the quality of those rays [Figure 19]. Not all of the x-rays produced were the same. Some were characterized as "soft," meaning that they were easily stopped by a barrier. These rays were absorbed by the skin but wouldn't penetrate more deeply into the body. Others were much "harder" and could penetrate deep into internal organs.

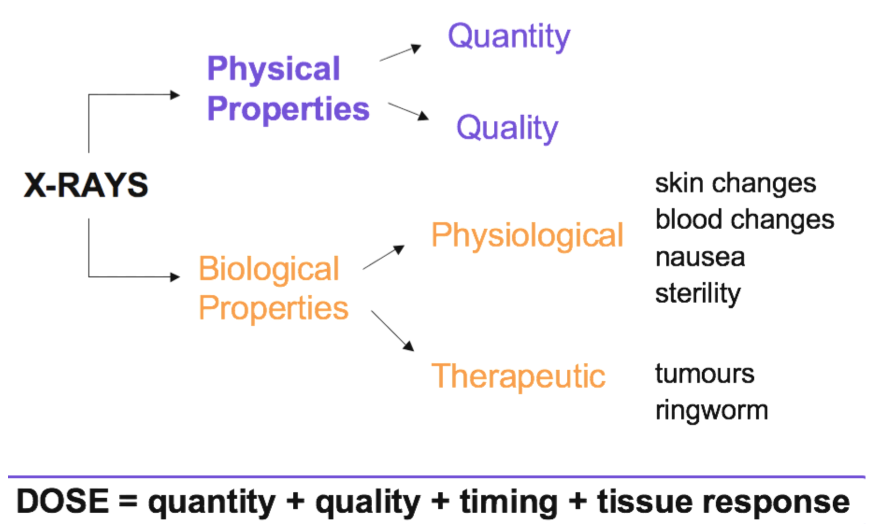

Figure 19: Specifying a Dose of X-Rays

[296] See Bruce Wheaton, *The Tiger and the Shark: Empirical roots of wave-particle dualism* (New York: Cambridge University Press, 1983).

In addition to the physical properties of the rays that had to be measured in some way, there were the biological properties that had to be understood. The first physiological effects noted were the ability of x-rays to cause hair to fall out and skin to redden. By the second decade of the twentieth century there was growing evidence from clinical research and animal experimentation that x-rays and radiation from radium disrupted white blood cell production.[297] Long sessions of x-ray therapy were known to cause a form of nausea that the Germans called "Roentgenkater," or x-ray hangover,[298] and there were reports of sterility in those operating x-ray tubes for long periods of time.[299] Like the reddening of skin that first hinted at the possible therapeutic benefits of x-rays, this last effect seemed to hold a silver lining for the many supporters of eugenics policies in this period, promising an "efficient, permanent and harmless," method of sterilization.[300] Some doctors even investigated x-rays as an abortifacient.[301]

More commonly, x-rays were utilized in the treatment of skin diseases and cancers. In 1904, the list of successful cases treated by x-rays included ringworm, eczema, and even superfluous hair as well as more serious conditions such as lupus, rodent ulcer and other

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[297] Hector A. Colwell and Sydney Russ, *Radium, X-rays and the Living Cell* (London: G.Bell and Sons Ltd., 1915).

[298] A. Béclère, "The "Penetrating-Irradiations-Sickness"," *American Journal of Roentgenology* 5(1918): 498.

[299] For a full review of experiments on animal testes and ovaries as well as published reports of sterility and subsequent birth defects in children of x-ray workers see Hector A. Colwell and Sidney Russ, *X-Ray and Radium Injuries: Prevention and Treatment* (London: Oxford University Press, 1934).

[300] "Rontgen Society and British Institute of Radiology Inaugural Meeting," 10.

[301] Tousey, *Medical Electricity, Röntgen Rays and Radium*: 1127.

malignancies.[302] There was no agreement as to the nature of the physiological processes underlying these therapeutic effects and the connection to the physical properties of x-rays was tenuous. Doctors knew that soft x-rays were better for treating skin conditions and harder x-rays were needed to reach internal organs. How hard or how soft varied between practitioners. The problem of dose, then, connected a number of complex variables. A full statement of dose would have to include the quantity of the rays produced, their quality, the timing—it was debated whether two one-hour sessions had the same therapeutic effect as one two hour session—and then a measure of how much radiation different types of tissue were able to absorb. Clearly not a simple problem.

3.4 Measuring Devices

The first call in Britain for a committee to be formed to determine a unit for measuring the radiation from an x-ray tube came from a physician, Dr. Deane Butcher, speaking at a meeting of the British Röntgen Society in 1904. As we have seen in Chapter 2, this was the first x-ray society to be formed in any country and from the beginning it welcomed all interested x-ray workers – doctors, physicists and manufacturers. When Dr. Butcher rose to speak of the need for consistent measurement, he was speaking to a room with representatives from each of these constituencies. Butcher was not concerned with the availability of measuring devices—by 1904 there were several methods of

302 C. Thurston Holland, "Presidential Address," *Journal of the Roentgen Society* 1(1904): 34.

measurement available—but the plurality and accuracy of these methods worried him. He argued:

> Much work remains to be done before we can even express ourselves in an articulate fashion, or transmit to others the clinical experience we have so painfully acquired, before we can assure ourselves that the Röntgen-rays of Vienna are comparable to the X-rays of America or be able to prescribe to-day a dose identical with that we gave a year ago.[303]

Figure 20: Dr. W. Deane Butcher, President of the Röntgen Society 1908-9.[304]

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303 Butcher, "The Means of Accurate Measurement in X-Ray Work," 75.

304 *Journal of the Röntgen Society* 6 (1910).

Many of his listeners agreed. In the words of Mr. Mulholland, accurate measurement was not only scientifically, but socially desirable:

> When a subject of study reaches the point of measuring up things, then it comes within the category of an exact science. It was the moment when electricians started measuring that electrical science emerged from the laboratory, into the arena of the commercial world and became of practical value to mankind. And so the effort to measure x-rays is not only a right one scientifically, but one that must lead ultimately to great value to the human race.[305]

Lorraine Daston has pointed out the historical plurality of these kinds of declarations, identifying accuracy, precision, communicability and impartiality as separate, though often related virtues of quantification.[306] In this conversation, Mulholland did not pause to reflect on what particular virtue or virtues of measurement would allow x-rays to be put to better use, but Butcher was clear. For him, consistent standards of measurement would allow for communicability, making it possible for doctors to share their clinical knowledge, and also for accuracy, allowing any doctor to consistently administer the same dose to multiple patients. It should be noted that Butcher was not expressing a desire for precision, the virtue of quantification that came to be most strongly advocated by the x-ray physicists. He was, however, endorsing impartiality, a virtue that stood in opposition to the

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305 Butcher, "The Means of Accurate Measurement in X-Ray Work," 79.

306 Lorraine Daston, "The moral economy of science," *Osiris* 10(1995): 8.

incommunicable clinical skill of the physician. Just as had been the case with the application of electricity to the body, those wishing to prescribe set rules for the application of x-rays faced two seemingly insurmountable problems: the complexity of the equipment and unexpected physiological reactions. In this meeting in 1904, Butcher dismissed the second problem, arguing that if there were unexpected reactions to x-rays, "The idiosyncrasy is not of the patient but of the doctor."[307] Butcher hoped to eliminate or at least reduce the idiosyncrasy present in physicians' therapeutic methods. But for many of his colleagues, this plurality of methods and even of results was to be expected: a byproduct of a type of medical practice that valued and trusted individual clinical skill. Earlier that same year, in his presidential address to the Röntgen society, the physician Thurston Holland had given an optimistic appraisal of the state of x-ray therapeutics, declaring, "the x-ray treatment of certain diseases has been established on a firm basis." For Holland, the firm basis of x-ray therapy was in no way threatened by the multiplicity of methods:

> The exact method, the best details of treatment, in each case must be left to the individual worker. Some seem to get their good results with what they call soft, some with hard tubes; length of exposure, distance of tube, size of coil, amount of current, and so on, all vary; but the broad result remains that all who use this method have their successful cases to record.[308]

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[307] Butcher, "The Means of Accurate Measurement in X-Ray Work," 86.

[308] Holland, "Presidential Address," 34.

But whether or not all physicians shared Butcher's vision of a standardized practice, the dangers posed by x-rays meant that the need for some kind of measure of the properties of the rays was universally acknowledged. And by the first years of the 20th century, there were multiple measuring devices offered, some of which caught on commercially.

Measuring Quality

The first device to measure the quality or the degree of hardness or softness of the rays was one which was used consistently into the 1930s by British and American radiotherapists. It was designed by a French physicist, L. Benoist in 1901 and consisted of a thin silver disc surrounded by 12 aluminum sections of varying thicknesses. X-rays were sent through the instrument and observation consisted of picking which aluminum section cast an image that was closest to the image cast by the silver circle. The harder the x-rays the thicker the section of aluminum that would match. Doctors could then specify that they were using x-rays that were, for instance, a #5 on the Benoist scale (Figure 21).[309] Numerous devices operating on this principle were marketed to doctors throughout the early 20th century. Figure 21 shows a penetration gauge marketed to American doctors by the Kny-Sheerer Company in 1905.

More complex electric measuring devices were also offered to measure quality but none were as popular as the basic penetrometers. In 1911, Butcher reported on Bauer's

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[309] X-ray measuring instruments are described in many textbooks from this time period. For a description of penetrometers see, for instance, P.K. Bowes, *X-Ray Apparatus: Its Arrangement and Use* (London: H.K. Lewis & Co., 1926), 96-97.

Qualimeter, an instrument for measuring the potential across the x-ray tube as a way of judging its hardness. Dr. Ironside Bruce reported that he had found the device useful, but it did not reach widespread use.[310]

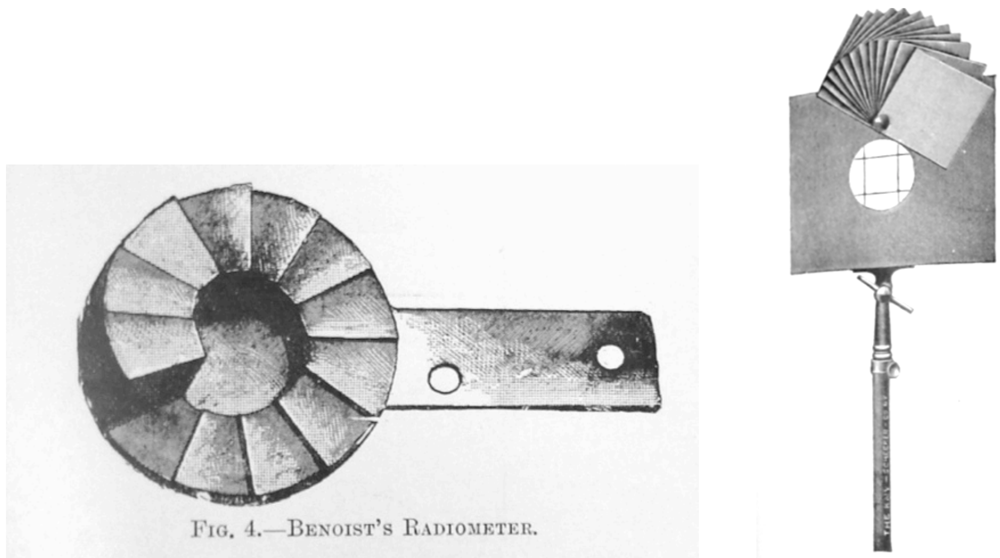

Figure 21: Benoist Penetrometer (left)[311] and X-Ray Penetration Gauge (right).[312]

While there was a continued insistence into the early 1930s that a measurement of the quality of the x-rays was crucial to a useful statement of dose, the question of a unit of quality did not occupy physicists the way that measures of quantity or intensity did. By

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310 W. Deane Butcher, "Bauer's Qualimeter: An instrument for determining the hardness of an x-ray tube by measureing the potential through the tube," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 4(1911).

311 David Arthur and John Muir, *A Manual of Practical X-Ray Work* (New York: Rebman Company, 1909), 7.

312 Kny-Sheerer Company, *Roentgen X-Ray Apparatus and Accessories*: 72.

1912, x-rays were shown to produce interference patterns just like visible light, and physicists agreed that the quality of the rays depended on their wavelength. There were various commercial spectrometers available to measure wavelength, and some were even marketed for medical use, but there is little evidence that doctors routinely measured wavelength as part of their everyday clinical practice.[313] The Benoist penetrometer and devices modeled after it remained in consistent use in x-ray therapy despite criticism from physicists. In the 1917 edition of his textbook on the physics of x-rays, Kaye pointed out that Benoist had based the theory of his instrument on faulty experimental data. The Benoist penetrometer used silver as a control and assumed that the transparency of silver did not depend on the quality of the rays. Kaye provided data showing that, in fact, the absorption of x-rays by silver varied considerably with wavelength.[314] Yet doctors remained content with the approximate measurements given by the device. When the new high voltage Coolidge tubes came into widespread use for deep therapy in the early 1920s, the question of measuring wavelengths was no long so pressing. These new tubes were so stable that doctors felt confident that the wavelengths they were producing were constant.

[313] Gebbert & Schall A.G. Reiniger, *X-Ray Spectrograph* (Erlangen192-).

[314] Kaye, *X-Rays*: 108.

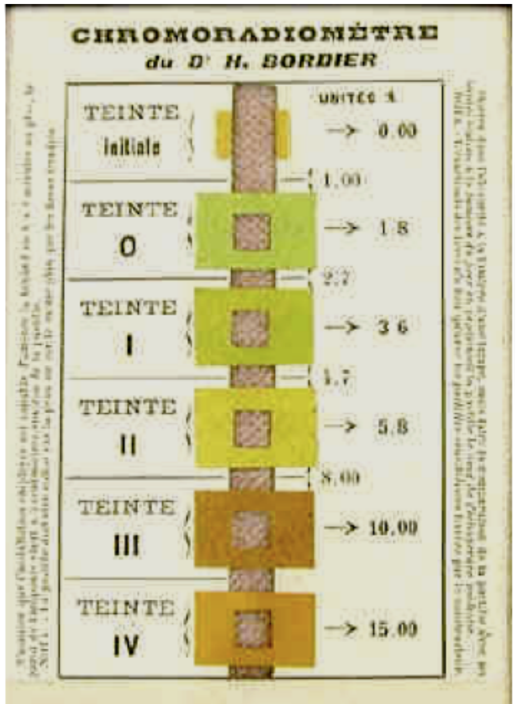

Figure 22: Chromoradiometer similar to that of Sabouraud-Noiré[315]

Measuring Quantity

By 1904, there were also several ways for doctors to measure the quantity or intensity of x-rays reaching the body. The most popular of these methods relied on the reaction of particular chemicals to x-rays. Barium platinocyanide, for instance, changed from a green to a yellow to a brown when exposed to x-rays and the most popular methods developed a scale based on this colour change (see Figure 22). Guido Holzknecht, an Austrian radiologist, invented a chromoradiometer in 1902 which consisted of a set of tablets that would change colour when exposed to the x-rays and a set of twelve test

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315 Oak Ridge Associated Universities (ORAU) Health Physics Historical Instrumentation Museum Collection, http://www.orau.org/ptp/collection/radiology/radiology.htm.

objects for comparison. According to the engineers at the Kny-Scheerer company in 1905, this was “a satisfactory and accurate measuring instrument.” With its invention, it “became possible for medicine to lay down exact statements.”[316] A similar scale was developed by Sabouraud and Noiré in Paris and this French scale became very popular with British doctors. These chemical scales were correlated with skin reactions so that one of the tints on the Sabouraud scale corresponded to 3 of Holzknecht’s units and 3H was defined as the amount of radiation required to induce a reddening of the skin or erythema.[317]

An alternate, though less popular method introduced by Robert Kienböck, another Viennese radiologist, measured the density of an x-ray image on a photographic plate or film as a measure of the quantity of x-rays being produced. The units were thought to compare so that 1H = tint B (on the Sabouraud scale) = 10 Kienböck units.[318]

But there was continued concern that individual bodies showed an idiosyncratic reaction to the rays. These various units were supposed to indicate to the doctor the minimum radiation required to induce a slight reddening, which was considered the safe dose limit. But Dr. Butcher, when reviewing these methods, urged British doctors to be very careful: “In my opinion these quantities should be used with great caution in this country, since an English skin appears to react much more readily than the darker integuement of France and Germany.”[319] Many doctors advocated using an actual patient as a standard to

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[316] Kny-Sheerer Company, *Roentgen X-Ray Apparatus and Accessories*: 10.

[317] Kaye, *X-Rays*: 98.

[318] Ibid.

[319] Butcher, "The Means of Accurate Measurement in X-Ray Work," 76.

determine the strength of a tube. In 1919, one American dermatologist advised using "a split-pea sized area of skin on the flexor surface of the forearm of a female adolescent (preferably a blond) for the experiment."[320] A blond teenage girl was chosen because her skin was thought to be the most sensitive. Doctors were then told to wait two weeks to see if any reddening appeared. If none did, then the radiologist could successfully increase the time of exposure and in this way determine the maximum safe dose for a particular tube.

Besides these chemical and photographic methods and the methods relying directly on observations of skin reactions, there were numerous others offered to the medical community. The American radiologist Sinclair Tousey presented a scale of Tousey units based on changes to a fluorescent screen.[321] Despite its wonderful name, the Tousey scale did not catch on. Other doctors hoped that they might be able to set up a scale using the reaction of a standard line of animal cells to the x-rays but that was never realized.[322] Some physicists noted that selenium cells changed resistance under the influence of the x-rays but a device based on this effect was never marketed.[323] The method that became the most popular in the United States was one that did not involve these kinds of direct measurements of the x-rays, but instead indirect measurements of the apparatus – the voltage across the tube, the current, the type of filter used, the skin to tube distance and the time of exposure. And finally, the method championed with greater and greater force by

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320 MacKee, "Arithmetical Computation of Roentgen Dosage," 603.

321 Sinclair Tousey, "An Intensimetric Scale for X-Ray Dosage," *The Journal of the Röntgen Society* 2(1906): 103.

322 Butcher, "The Means of Accurate Measurement in X-Ray Work," 82.

323 Albert Bachem, *Principles of X-Ray and Radium Dosage* (Chicago: Albert Bachem, 1923), 65.

physicists was one based on measurements of the ability of a particular beam of x-rays to ionize air.

3.5 Standards Committees

Given this multitude of options, both the British and American communities established committees to look into the problem with the hope of settling on a unit with which to measure both the radiation from radium and from x-rays. These committees worked on and off for two decades, providing a forum for the discussion of physics research into the problem of measurement. Physicists increasingly championed ionization methods, building up a long list of criticisms for the alternative methods of measurement preferred by their colleagues in medicine. But these colleagues proved resistant to their recommendations.

Dr. Butcher first broached the idea of a committee to determine a unit of radiation in 1904,[324] and put forward a formal motion at a subsequent meeting of the Röntgen Society. At this time the primary discussion centered, not around x-rays, but around a radium standard that could be housed at the National Physical Laboratory.[325] The problem of a radium standard was one that appeared much more easily solved. At the Röntgen Congress in Berlin in 1905, 500 doctors, physicists and electricians from Europe, Britain, the United

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[324] Butcher, "The Means of Accurate Measurement in X-Ray Work," 74-86.

[325] ———, "Discussion on the Need for a Radio-active Standard," *Journal of the Röntgen Society* 2(1905): 93-100.

States and Mexico met to discuss current research in radiology. One doctor suggested that a committee be formed to choose a universal unit of x-ray measurement. The congress decided to consider it, "in spite of the opinion of some members that the time was hardly ripe for such a step."[326] An international committee to look into the problem of measuring radiation was eventually formed at the Congress of Radiology at Brussels in September 1910 with Rutherford as President. In March 1912, the committee adopted 21.99 mg of pure radium chloride prepared by Marie Curie as the international radium standard. But the problem of x-ray measurement remained unsolved.[327]

Butcher's motion to form a British committee was carried at an Ordinary General Meeting of the Röntgen Society on April 5, 1906. On July 23, 1906, the first meeting was held. This committee was composed overwhelmingly of physicists and included a number of prominent names: William Crookes, Oliver Lodge, Silvanus Thomson, and Ernest Rutherford.[328] In the United States, the British physicist C.E.S. Phillips brought attention to the problem and stimulated a vibrant discussion, reading a paper on "Standardising Radiations" for the American Roentgen Ray Society (ARRS) in April, 1906.[329] At their annual meeting that August the ARRS appointed their own committee, composed of 5 physicians, to confer with the British committee.[330] The American committee does not

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[326] "The Röntgen Congress at Berlin," *The Journal of the Röntgen Society* 2(1905): 8.

[327] Kaye, *X-Rays*: 88.

[328] "Notes," *Journal of the Röntgen Society* 3(1906): 48.

[329] C.E.S Phillips, "The Standardisation of Radiations," *American Quarterly of Roentgenology* 1(1907).

[330] "Minutes of the Proceedings of the Seventh Annual Meeting Held at Niagara Falls, N.Y," *American Quarterly of Roentgenology* 1(1906): 78. The doctors were Johnston, Leonard, Dunham, Caldwell and Williams. This committee was reported on and praised by the Röntgen Society. "Interim Report of the Standards Committee," *The Journal of the Röntgen Society* 4(1908): 28.

appear, however, to have been very active. After this initial announcement, there is not mention of it in the publications of the ARRS in the following years. North American doctors, however, continued to express their desire for standardized measurement. After hearing a paper and participating in a long discussion on the problem of dosage, Dr. G.P Girdwood, in 1907, declared, "I would like very much to have a unit; in fact, that is what we all want."[331] And yet, when the physicist S.J. Allen, rose to present his research on ionization measurements at the same meeting, no doctors weighed in to discuss the paper. Unlike the clinical papers which sparked lively discussion, there was only one commentator, Mr. Snook, a fellow physicist.[332] The separation between the medical and physical communities in the United States continued to be strong. The next formal committee to consider the problem of dosage in North America wasn't formed until 1925.[333]

The British committee, on the other hand, kept quite busy and when the international community settled on a radium standard, a new committee was formed in 1913 to look into X-ray measurement and dosage exclusively.[334] Like the committee it replaced, this one was composed overwhelmingly of physicists. Of the 8 members, the two physicians, Drs. Butcher and Knox were well outnumbered by physicists who included C.E.S Phillips, Sidney Russ and G.W.C. Kaye. In the following years, often under the supervision of

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331 Ennion G. Williams, "The Regulation and Measurement of the Therapeutic Dose of the Roentgen Ray," *Transactions of the American Roentgen Ray Society: Sixth Annual Meeting* (1907): 93.

332 S. J. Allen, "A Null Instrument for Measuring Ionization," *Transactions of the American Roentgen Ray Society: Sixth Annual Meeting* (1907): 237.

333 This was the Standardization Committee of the Radiological Society of North America, discussed below.

334 "Röntgen Society 17th Annual Report," *Journal of the Röntgen Society* 10(1914): 90.

these committees, quite a bit of research was undertaken by physicists investigating the strengths and weaknesses of the available methods of measurement.

This research was presented to the Röntgen Society on a regular basis and the conversations that took place following the presentation of these papers illuminate the divergent values of the different x-ray constituents. Tensions were clearly evident in the discussion that took place following the presentation of one of these physics papers in 1906. The paper was presented by two physicists, Lord Blythswood and Walter Scoble, who were investigating the Kienboeck photographic method. Blythswood and Scoble were quite critical of the method, pointing out that errors were introduced because of differences in the development of different films and because of the personal error involved in matching the developed film to the standards for comparison.[335] A mixed group of physicists and doctors responded to the paper with very different reactions. C.E.S Phillips, a physicist, was one of the first to speak, agreeing with the authors of the paper that: “the method of Kienboeck does not seem to go far.”[336] But Dr. Butcher, our doctor who remained committed throughout his career to the establishment of consistent measurement standards, jumped in to defend the Kienboeck method saying, “It is not designed as a scientific instrument for use in the laboratory, but as a practical instrument

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[335] Lord Blythswood and Walter A. Scoble, "A Comparison of the Sensitiveness of Photographic Plates to Light and the Röntgen Rays," *The Journal of the Röntgen Society* 3(1906): 38-47. These authors published further research on the problem of personal error in ———, "A Test of Kieboeck's Quantimeter," *The Journal of the Röntgen Society* 3(1907): 36-38.

[336] ———, "A Comparison of the Sensitiveness of Photographic Plates to Light and the Röntgen Rays," 38-47.

for use by the physician."[337] Another physician, Dr. Batten, dismissed entirely the problem of personal error that had been raised by the physicists:

> I do not think that matters at all from our point of view. We have each to learn his own apparatus, and we really get to depend upon our own observations as a rule, and not upon those of other people.[338]

Mr.J.H. Gardiner, another physicist,[339] ended the conversation clearly expressing disapproval over the attitudes of his colleagues in medicine:

> I am pleased to find that the original aim of this Society is being carried out in these later days, and that physicists and medical men are cooperating so wholeheartedly. It seems to me, however, that the medical section shows a tendency to be rather too easily satisfied with approximate methods of measuring. As a physicist one cannot be content with approximations.[340]

We can see already in this conversation a clash of the values and beliefs that would come to define the interactions between doctors and physicists for the next decades: For the physicists, this included a desire for precision and quantification, for objective methods

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[337] Ibid., 42.

[338] Ibid.

[339] J.H. Gardiner was an assistant to William Crookes from the 1880s to the 1910s and was a frequent contributer to the *Journal of the Röntgen Society*. S.R., "James H. Gardiner," *British Journal of Radiology* 19(1946): 348.

[340] Blythswood and Scoble, "A Comparison of the Sensitiveness of Photographic Plates to Light and the Röntgen Rays," 43.

that removed altogether the personal attributes of the measurer, and a belief in a body that could ultimately be standardized and normalized.

As we will see even more clearly in the following chapter, physicians resisted these values, trusting the expertise of individual doctors and maintaining their belief in an idiosyncratic body. But in addition, these doctors valued a pragmatism that was often born simply out of the realities of working in a busy hospital. Many busy hospital radiologists found even the chemical and photographic methods of measurement to be too cumbersome. In hospitals treating hundreds of cases a year and multiple cases a day, the best that they could do was keep the voltage, current and distance constant and record the time of exposure. In a paper reporting successes in the treatment of exophthalmic goitre in 1904, Thurston Holland recorded that his technique in each case was to give the patient a 5 to 10 minute exposure. Holland apologized that "a statement of this kind conveys no information of any sort to other workers." But defended himself:

> The question of exposure, and measuring it, is of some importance, but, from a practical point of view, in a large and busy department responsible for *all* the electrical treatment of the infirmary, when thirty or forty cases a day often attend for treatment alone, when about 1500 cases of all sorts are dealt with yearly for X-ray diagnostic purposes, when the instrumentation is totally inadequate and hopelessly out of date, and when the work is being constantly

> interrupted in all kinds of ways, it has been found absolutely impossible to do anything else as a routine measure than to estimate our exposures by time.[341]

Physicists' wish for greater precision and standardization appeared impossible in an overworked hospital department.

We should notice, as well, that this movement for standardization first took place in the Röntgen Society, a professional body full of non-medical members—so full in fact that a purely medical branch had split off in 1902 because it felt overwhelmed (see Chapter 2). Dr. Butcher was a surgeon with no special training in physics, but in calling for standard procedures and consistent measurement he had come to share at least some of the goals of the physicists—their desire for standardized measurement if not their desire for precision. The boundaries that I have been delineating—between physics and medicine—become blurry in the fine details.

But even so, Butcher had a clear vision of the difference between instruments for lab purposes and those, "for the rough measurements such as a doctor needs." When the Standards Committee of the Röntgen Society made their interim report in 1908, Butcher clearly worried that the research was becoming too abstract:

> I am desirous that in all we do we should bear in mind the fact that our only reason for making these investigations is to obtain a practical standard which shall be useful for therapeutic purposes. It is true that we are a physical

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[341] Holland, "Presidential Address," 34.

> society, but after all, we are not so much interested in the abstract physical questions as in the production of a practical means of measurement.[342]

And Dr. Butcher expressed frustration, not only with his colleagues' fixation with precision measurement, but with their preoccupation with questions of ontology. In his mind, this preoccupation meant that the non-medical men were overestimating the magnitude of the task: "There seems to be an idea outside this Society that we are attempting to do something impossible or in its nature inherently absurd."[343] He quoted a friend whom he called "a distinguished electrician":

> There are the activities of the rays we know, the rays we do not know, the rays we dream of, and the rays we do not even dream of. They do not end with the cathode ray, the vibrations projected by radium, or even with the movements of men's thoughts …[344]

But Butcher is pragmatic: "It is not absolutely necessary to know everything about a physical force in order to measure it for technical purposes. Had we waited to measure light until we knew all about the ethereal waves, we might have waited forever."[345]

The eventual adoption of a unit based on ionization measurements did not require the physics community to commit to a particular model of x-rays but it did represent a victory for their commitment to precision. And, as we will see in the next chapter, this

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[342] "Interim Report of the Standards Committee," 33.

[343] Butcher, "Discussion on the Need for a Radio-active Standard," 93.

[344] Ibid.

[345] Ibid.

victory came at a time when doctors were increasingly satisfied with the alternative measurement techniques they had come to adopt.

4. Measuring X-Rays

Medical Measurements—Unquantifiable Doctors and Bodies—Pastilles and Ammeters—Physics Insistent—Enter the Röntgen—Resistance and Adoption—Conclusion

"If we talk of measured dosage, do we not perhaps err in putting too much emphasis on the idea of a 'cancer dose' when we all know that cancer differs as widely in reaction as do patients in disposition, and that any claim of absolute accuracy in dosage must cease when the beam of rays strike the skin, since from that point, the unknowns obscure all formulae?"[346]

Dr. C.D. Enfield, *Radiology* (1923)

4.1 Medical Measurements

The British surgeon Louisa Martindale[347] treated 118 cases of uterine fibroids between 1914 and 1920 (Figure 23), and in each case she faithfully recorded the delivered dose of x-rays using photographic strips measured with a Kienboeck quantimeter, noting:

> My nurse carefully labels each strip and places it on the patient, and at the end of each day develops them in the standard developer for specified time at a specified

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[346] C.D. Enfield, "What the Neighbours Think," *Radiology* 1(1923): 181.

[347] Martindale is remembered as a pioneering female physician. Her autobiography, *A Woman Surgeon,* was published in 1951.

> temperature. I have therefore accurate and permanent records of the dosage employed for each patient.[348]

We saw in the last chapter that physicists routinely criticized this method of measuring x-rays, dismissing the photographic record as too approximate and subjective. Yet Dr. Martindale and her nurses trusted this method and faithfully recorded dosage in Kienboeck units for over 6 years. Each patient received a 3 to 7 minute exposure (depending on the type of x-ray tube) on each of 22 areas drawn on the front and back of the abdomen (Figure 24). The total therapy took 2 to 4 hours in two consecutive treatments spaced three weeks apart.

In her published account of these cases, Dr. Martindale outlined her criteria for choosing radiotherapy over surgery, but did not illuminate the reasoning behind the very different dosages given for seemingly similar cases. In choosing x-ray therapy over a hysterectomy, Dr. Martindale explained, "I have tried as much as possible to let *the physical signs and symptoms* be the determining factor."[349] The size of the tumour and its location were her main considerations but underlying health concerns could also play a role. Individuals with heart disease, for instance, were not recommended for surgery. Profession and expense often also determined the course of treatment. Patients with demanding occupations, including a number of headmistresses, opted for x-ray therapy rather than the surgery because the menopause that was produced resulted in fewer symptoms and therefore did not interfere with their work.

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[348] L. Martindale, "Intensive X-ray Therapy versus hysterectomy for Fybromyomata of the uterus," *Archives of Radiology and Electrotherapy* 25(1920): 98.

[349] Ibid., 101.

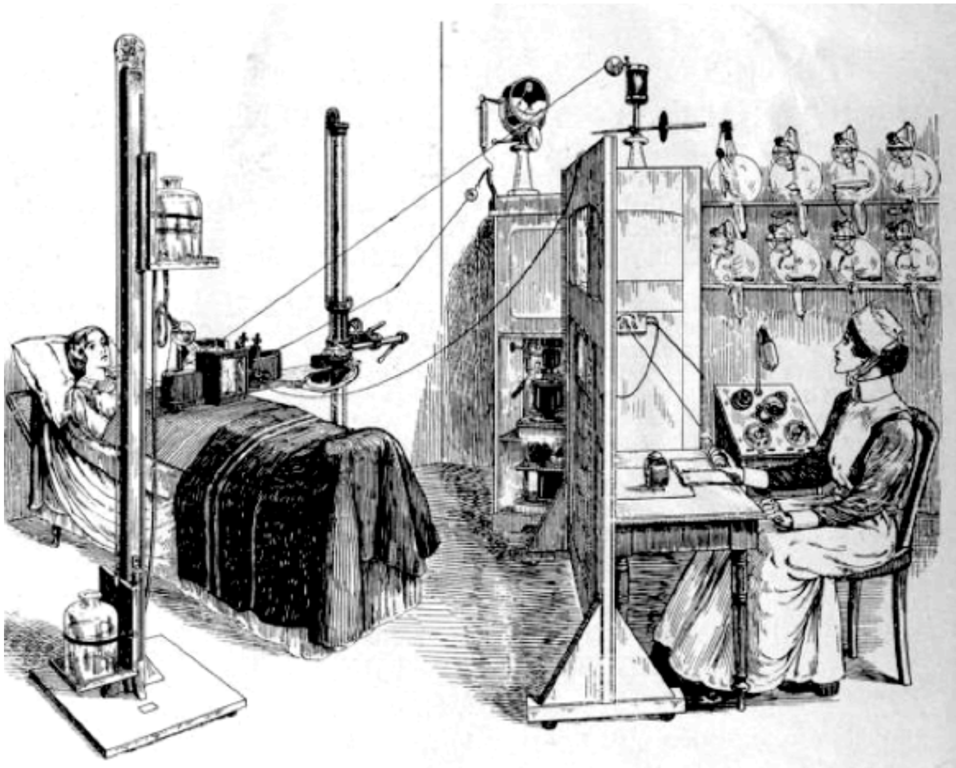

Figure 23: X-Ray treatment for uterine fibroids c. 1918[350]

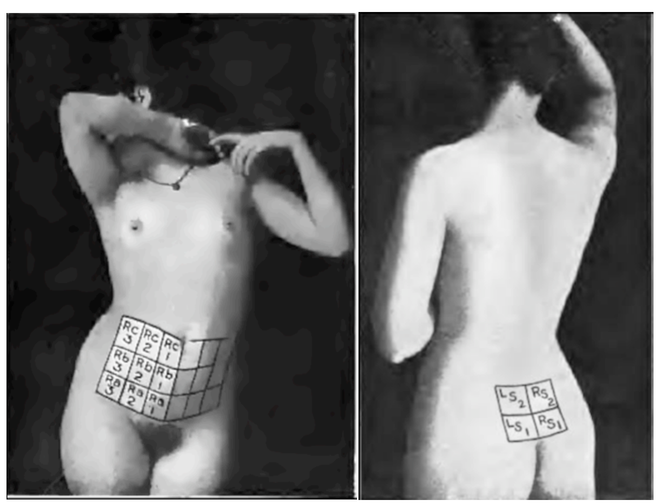

Figure 24: Grid on patient showing areas irradiated in x-ray treatment of uterine fibroids.[351]

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[350] Ibid.

X-RAY TREATMENT OF FIBROIDS, JUNE, 1920.—*Continued.*

No.	Name	Age	Occupation	Date	No. of Children	Symptoms	Size of uterus (in terms of pregnancy)	Kind	No. of treatments	After Results	Other doctors who had examined the case	Dosage (Kienboeck)
20	Miss M.	48	Headmistress...	1916	—	Menorrhagia and metrorrhagia	4½	Interstitial (tenderness over right ovary)	12	Treatment from Jan.-July, 1916; and in Dec., 1916, and in Dec., 1917; but M.P.'s continued slightly till early 1919. In July, 1918, Miss Aldrich Blake saw her, and was in favour of a hysterectomy, on account of metrorrhagia, but patient would not consent. I curetted, and had curettings examined; they were non-malignant, 1920, patient very well indeed; amenorrhœa for last year	Miss Aldrich Blake	4435 x
21	Mrs. M.	35	—	1914	4	Menorrhagia and metrorrhagia	2½	Small sub-peritoneal fibroid on post wall	5	M.P.'s normal, but no amenorrhœa	Dr. Macgregor	1095 x
22	Miss M.	42	—	1914	—	Menorrhagia	8	Interstitial and sub-peritoneal	11	Amenorrhœa after 4th treatment. Measurement from top of tumour to sym. pubis, 8 inches; at end of treatment, 5 inches	Dr. Scatliff	2257 x
23	Lady N.	46	—	1915	4	Menorrhagia	3	Interstitial ...	6	Amenorrhœa after 3rd treatment; tumour decreased. 1920 letter confirms cure	Miss Aldrich Blake	2726 x
24	Mrs. P.	49	Matron ...	1916	0	Menorrhagia	4	Interstitial and post sub-peritoneal fibroid also	7	Amenorrhœa after 4th treatment. Reduction in size of tumour. 1920 letter confirms cure	Dr. Shearer. Dr. Russell Andrews examined patient in 1917 and confirmed cure	2830 x
25	Miss P.	42	—	1919	—	Menorrhagia, dysmenorrhœa, frequency and pain on micturition	3½	Interstitial and ? right ovary	9	Menorrhagia continued, anæmia, dilated heart, tachycardia, and could not continue treatment	In 1917, Dr. Handfield Jones did a dilatation and curretting. In 1919, both he and Col. Jowers agreed as to X-ray treatment. Dr. Broadbent also saw case	5665 x
26	Mrs. P.	44	—	1919	2	Menorrhagia	2½	Fibroid interstitial	6	Amenorrhœa after 4th treatment. 1920 letter confirms cure	Dr. May Thorne	4840 x

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Figure 25: Case histories for a selection of Dr. Martindale's patients. The dose in recorded in the far right hand column. X is the unit of measure for the Kienboeck method.[352]

In addition, x-ray therapy remained much more expensive than surgery and was not an option for her poorer patients. Given the relevance of all of these complicated factors, Martindale did not provide any definitive set of rules to guide her fellow surgeons in

[351] Ibid.

[352] Ibid.

choosing the appropriate course of action. And for her colleagues choosing to pursue x-ray therapy, she only provided a rough guide to the determination of an appropriate dose. We can see in Figure 3 that the total dose delivered to each patient as measured by the Kienboeck strips differed quite drastically from one case to the next. Lady N, for example, was given a total dose of 2726 X (in Kienboeck units) over 6 sessions for an interstitial fibroid, while Mrs. P was given 4840 X over the same number of sessions. Both were in their mid-40s and both had the same type of fibroid.

Charles Hayter has taken a similarly close look at the use of radium by the Canadian doctor, William Aikin, in the first decades of the 20th century, and has concluded that the only way to understand what he calls Aikin's "apparently erratic dosing" is to see the "emphasis on individual patient experience … rather than some standard dose based on the average outcome in a group of patients."[353] Aikin prescribed and altered doses based on the physical reaction of each of his patients individually. Dr. Martindale was doing the same. If we look closely at the 'results' column for Mrs. P and Lady N, we can see that Lady N exhibited amenorrhea after 3 doses while this result was not achieved for Mrs. P until 4 doses had been administered. Martindale did not draw attention to this difference in her published account but we can guess that she altered her treatment plan for each patient in reaction to these kinds of exhibited responses. Just as Dr. Enfield proclaimed in the quote at the start of this chapter, there was no 'uterine fibroid dose' any more than there was a 'cancer dose.' The information presented in her case histories allowed Martindale to publicly establish the efficacy of her treatment, but she did not expect her colleagues to

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[353] Hayter, *An Element of Hope*: 29. See also ———, "The Clinic as Laboratory: The Case of Radiation Therapy, 1896-1920," *Bulletin of the History of Medicine* 72, no. 4 (1998): 663-88.

follow her dosing scheme exactly. Her measurements were performed mainly for the sake of her own practice, allowing her to keep track of the amount of radiation applied, so that she could adapt her therapeutic plan to suit the needs of each patient.

A generation before randomized clinical trials would become standard practice for evaluating new therapies, clinical research in medicine inhabited what Harry Marks has called “a culture of individualism.”[354] Authority was rooted in individual clinical experience, making cooperative, collaborative research programs difficult to coordinate. Radiotherapeutic research was no different. Physicians investigating the therapeutic effects of x-rays did so on their own, developing their own personal methods of applying the radiation to their patients.

Within this culture, quantitative measurement fulfilled a particular function. Theodore Porter has argued that numbers act as a “technology of distance,” allowing for communication of scientific results beyond a local context; that they “minimize the need for intimate knowledge and personal trust …”[355] Yet these most commonly touted virtues of quantification—communicability and impersonality—appear somewhat at odds with the culture of individualism in early 20th century medicine. I will argue that quantification took on a different set of virtues in this culture. Physicians like Dr. Martindale adopted particular measuring devices to characterize, quantify and record properties of the x-rays they used – not because these measurements acted to suppress their subjective self or in order to pass

[354] Randomized clinical trials did not become the gold standard in clinical research until the 1950s. See Harry M. Marks, *The progress of experiment: science and therapeutic reform in the United States, 1900-1990*, Cambridge history of medicine. (Cambridge [England] ; New York: Cambridge University Press, 1997).

[355] Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*: ix.

on impartial information to their colleagues, but because they were deemed useful to the development of their own clinical art. The numbers created by these measuring devices did not act primarily as technologies of distance but rather as technologies of introspection, allowing doctors to monitor and modify their own practice. If the judgement of a particular colour on a pastille or shade on a photographic strip was subjective and imprecise, these were not fatal flaws, as long as the same doctor could repeat the measurement to his or her own satisfaction.

Given this culture of individualism, it is not surprising that the push for standards and uniform practice in the early 20th century came largely from outside of clinical medicine, spurred by the interests of life insurance companies and later, regulatory bodies like the FDA.[356] For the doctors involved in radiotherapy, the single biggest source of pressure to adopt uniform methods and standardize their practice was also external – coming from their colleagues in physics.

This chapter will expand on the themes of the last chapter, showing how a continued respect for the clinical art and a belief in the idiosyncratic body nurtured significant resistance to the rhetoric of scientific medicine in doctors practicing radiotherapy in both the United States and Britain. Just as practice stabilized in these communities, a handful of vocal x-ray physicists began to call for a new unit to measure x-ray quantity, despite overwhelming evidence that the doctors themselves were quite content with their chosen measuring devices.

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[356] Ibid., 206.

4.2 Unquantifiable Doctors and Bodies

We saw in the last chapter that a doctors' choice of measuring device was constrained by the realities of busy practice. Hospital doctors offering x-ray therapy were often only able to measure the time of exposure with a particular tube, hoping that that tube would stay at least fairly consistent in its output. But even without these constraints, the precision ionization devices preferred by x-ray physicists were simply viewed as unnecessary. The doctors involved in radiation therapy expressed again and again a deep belief in the individual skill of each practitioner - the clinical experience or art that developed after years of practice, that rendered communication of exact method impossible and even undesirable. In their review of their technique for treating exophthalmic goitre in 1920, Drs. Burrows and Morison wrote:

> It is not necessary to describe in full the apparatus used and the technique employed by radiologists. All kinds of apparatus and different types of tubes appear to give equally good results in skilled hands.

These doctors went on to say without apology that their own technique "varies considerably … The question of dosage and filtration must be decided by the radiologist according to the needs of the particular case."[357] By the 1920s, British and American doctors administering therapeutic x-rays had had time to establish their own measuring techniques, adapted to the demands of their practice. Exact details of these techniques

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[357] Arthur Burrows and J. M. Woodburn Morison, "The Treatment of Exophthalmic Goitre by Radiation " *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 13(1920): 134.

became less and less frequent in the published literature until many of the published accounts of therapeutic success came to contain none of the particulars of the actual treatment.[358] Without pressure to communicate technique for the sake of standardization, the exact details of measurement, dosing and of the equipment being used began to disappear from journal articles.

Intimately connected to this faith in individual styles of medicine was a continued sense of the idiosyncratic body. Unpredictable doctors and unruly bodies worked together to make a detailed statement of methods irrelevant. The idiosyncrasies identified by radiotherapists ranged from observations of the peculiar sensitivity of blonde adolescent girls,[359] to fundamental differences in how the same disease responded to x-ray treatment in different patients. Historians have traditionally characterized the 19th century as the time in which a holistic, humoural body was replaced in medical theory by normalized bodies responding uniformly to specific disease entities. Accompanying this shift in medical epistemology was a shift in the identity of physicians. With bodies newly constrained by the strict physiological models of laboratory medicine, doctors needed scientific knowledge even more than clinical experience. John Harley Warner has argued that, “The defining core of the proper physician’s task became less the exercise of judgement and more the expert

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358 See for instance Emil G Beck, "The Open Method of Surgery in Deep-Seated Recurrent Cancer Prepatory to Roentgen and Radium Therapy," *American Journal of Roentgenology* 6(1919): 559-65. and J.H. Webster, "Roentgen Ray Treatment of a Case of Early Acromegaly," *Archives of Radiology and Electrotherapy* 24(1919): 261-64.

359 X-rays both conformed to and threatened entrenched racial categories. It was generally accepted that lighter skinned patients were more sensitive to radiation than those with darker skin but x-rays were also observed to lighten skin tone. In one case early in the century this led to widespread newspaper reports that x-rays had bleached the skin of a black patient. See Caroline Thomas de la Pena, ""Bleaching the Ethiopian": Desegregating Race and Technology through Early X-Ray Experiments," *Technology and Culture* 47(2006): 27-55.

application of knowledge."[360] In his study of blood disorders in the 20th century, Keith Wailoo has similarly argued that doctors transformed from 'moral managers' to what he calls 'clinical managers' as lifestyle factors ceased to be relevant in the diagnosis and treatment of anemia.[361] Given this characterization, both the emphasis on individual styles of medicine and the continued, widespread belief in unpredictable individual responses to radiation on the part of radiologists, may appear surprising. But as a number of historians have demonstrated, there was significant resistance in the early 20th century to this new vision of scientific medicine, both to the direct applicability of the natural sciences[362] and to the perceived suppression of clinical experience.[363] Resistance to scientific medicine continued into the interwar period with renewed condemnations of reductionism from a diverse group of practitioners and medical scientists in Europe and North America in the 1920s and 30s.[364] Christopher Lawrence has identified a group of elite doctors in Britain in this period, 'patrician' physicians, often with close ties to the British aristocracy, who were united in their denunciations of standardization and mechanization in medicine. These doctors tended to cling nostalgically to an older ideal of the gentlemanly clinical art.[365] In

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[360] Warner, *The Therapeutic Perspective: Medical Practice, Knowledge and Identity in America*: 260.

[361] Keith Wailoo, *Drawing Blood: Technology and Disease Identity in 20th Century America* (Baltimore: Johns Hopkins University Press, 1997).

[362] See Rosenberg, "The Therapeutic Revolution: Medicine, Meaning and Social Change in 19th Century America," 3-26. and Geison, "Divided We Stand: Physiologists and Clinicians in the American Context," 67-90.

[363] Lawrence, "Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain, 1850-1914," 503-20.

[364] George Weisz and Christopher Lawrence, eds., *Greater than the Parts: Holism in Biomedicine 1920-1950* (Oxford: Oxford University Press, 1998).

[365] Christopher Lawrence, "Still Incommunicable: Clinical Holists and Medical Knowledge in Interwar Britain," in *Greater than the Parts: Holism in Biomedicine 1920-1950*, ed. George Weisz and Christopher Lawrence (Oxford: Oxford University Press, 1998), 94-111.

fact, a number of historical episodes throughout the mid-twentieth century, including the Tuskegee Syphilis Study, cast doubt on the assumption that 20th century western medicine whole-heartedly adopted the model of one standard human body.[366] In her study of Spanish radiotherapists, Rosa María Medina Doménech, argues that even while these doctors appealed to the rhetoric of scientific dosing to establish their legitimacy, they resisted the idea of a standard technique and continued to emphasize the importance of "trained judgement."[367] This chapter will add the voices of a diverse cross section of British and American radiologists who continued to debate the extent of idiosyncratic reactions of different bodies to x-rays, just as they continued to emphasize the need for individual judgment in the treatment of patients by radiation.

By the first decades of the 20th century, some radiologists *had* clearly adopted a belief in a standard body. The British doctor, Deane Butcher, for instance, tended to downplay the problem of idiosyncrasy:

> Although there may be racial differences, and differences due to sex and age, we shall soon be able to give a dose of x-rays with as much certainty as we can give a

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[366] The Tuskegee Syphilis study, undertaken from the 1930s to the 1970s, was motivated in part by a desire to study the course of the disease in a black population. In his recent book, Steven Epstein points out that the idea of a standard body is a very recent medical concept. For most of Western medical history, doctors have assumed the medical relevance of categories of race and gender. Epstein points out the danger in emphasizing categories of difference in medical research. "While today's presumption is that medical attention to difference is a beneficial and enlightened response, more typically such attention has both presumed and reinforced a social hierarchy that placed heterosexual European men at the pinnacle." See Steven Epstein, *Inclusion: The Politics of Difference in Medical Research* (Chicago: University of Chicago Press, 2007)., quote on p.34.

[367] Doménech, "Scientific Rhetoric in the Consolidation of a Therapeutic Monopoly," 221-42.

> dose of morphia. There are doubtless cases of idiosyncrasy for x-rays as there are for morphia, but these are so rare that we may safely neglect them.[368]

Some researchers investigated alternate explanations for the diverse range of observed reactions to x-rays. The American doctors, Charles Martin and George Caldwell, for instance, investigated the extent to which the variation in skin changes after exposure to x-rays might be due to differences in the temperature of the skin being irradiated, rather than differences in individual susceptibility to the radiation.[369] But a large part of the radiological community continued to assume that the explanation for the wide variety of patient responses lay in the patients themselves and not in external physical factors. Dr. Beck, a surgeon, and Dr. Eison, a roentgenologist to North Chicago Hospital, felt that different reactions to both cancer and x-rays made a standard technique impossible:

> The time of exposure depends upon the depth to which the cancer reaches and upon the toleration of the patient to the rays. It is therefore obvious that no two cases can be treated alike, and each individual application is dependent upon the patient's general condition.[370]

Dr. H.J. Ullmann, a physician at Santa Barbara Cottage Hospital addressed this problem in a paper entitled, "Radiation Dosage: Standardization versus individual adaptation," and concluded forcefully that, "anything like a uniform standard reaction of

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[368] Butcher, "The Means of Accurate Measurement in X-Ray Work," 86.

[369] Charles L. Martin and George Caldwell, "The Relation of Temperature Changes to Roentgen Ray Skin Reactions," *American Journal of Roentgenology* 9(1922): 152.

[370] Paul Eisen, Emil G Beck, and G Warner, "Technique of Radiotherapy," *American Journal of Roentgenology* 6(1919): 567.

the tissues [to x-rays] does not exist."[371] He went on to blame the false promise of physics for impeding x-ray therapy:

> The attempt to substitute physics for biology and medicine can only prove an obstacle in the way of sound progress in radiotherapy … we must stop the search for the carcinoma dose as expressed in physical factors of wavelength and intensity alone.[372]

Even if physicists and engineers could standardize the physical factors, Ullmann argued that doctors would still need to apply these factors to suit the needs of each individual. He ended the piece entreating doctors to "think in terms of the body as a whole."

We have already seen this attitude clearly displayed in Dr. Martindale's treatment of uterine fibroids, and it was a therapeutic attitude backed up by physiological research. Hector Colwell and Sidney Russ, a surgeon and a physicist to Middlesex Hospital, respectively, published two of the most comprehensive works reviewing research into the physiological actions of x-rays and radium. In their first book, *Radium, X-Rays, and the Living Cell* (1915), Colwell and Russ acknowledged that there was no consensus among radiologists as to the extent of idiosyncratic reactions to x-rays, but concluded that:

> There is little reason to doubt that an idiosyncrasy actually exists in the sense that the same dose of radiation will provoke a reaction, the degree of which varies with

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[371] H.J. Ullmann, "Radiation Dosage: Standardization versus individual adaptation," *Radiology* 1(1923): 32.

[372] Ibid.

> the individual. Moreover, the same person may react at different times to a varying degree when exposed to the same dose of rays…[373]

Even into the 1930s, Colwell and Russ emphasized unpredictable patient reactions. In their second work, *X-Ray and Radium Injuries* (1934), they continued to attribute certain radiation injuries, not to imperfect dosing techniques, but to "some idiosyncracy of the patient."[374]

The sustained insistence of radiologists that the body was unpredictable and that their special expertise was crucial to their practice must be understood in light of their struggle for professional recognition within medicine. In Chapter 2, we saw how concern over the status of radiology within medicine as a whole motivated calls in both the United States and Britain for postgraduate education in the new specialty. Those practicing radiotherapy were preoccupied in particular with their relationship to surgeons. While some surgeons, including Dr. Martindale, practiced both surgery and radiotherapy, other surgeons looked with suspicion on the new specialty that threatened to encroach on their territory.[375] In a letter to the editor, entitled "What the Neighbours Think," in 1923, Dr. Enfield wrote that the value of a radiologist was "directly proportionate to his ability to work in harmony with the surgeon and internist." Enfield, an American roentgenologist, felt that his work was "under jealous scrutiny" from these colleagues:

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[373] Colwell and Russ, *Radium, X-rays and the Living Cell*: 287.

[374] Colwell and Russ, *X-Ray and Radium Injuries: Prevention and Treatment*: 3.

[375] Moscucci, "The "Ineffable Freemasonry of Sex"," 139-63.

> Have we, perhaps, allowed the machine and its output of rays to be emphasized too much, and put too little stress on the experience and intelligence which must be behind it to ensure worthy results? One hears some very caustic surgical comment on dosage charts, which seems not without merit.[376]

Enfield was echoing the concerns of earlier electrotherapists who had worried that their expertise was perceived to lie in their batteries rather than in themselves. These early 20th century radiotherapists were similarly worried that their own clinical skill was being overshadowed by the machines they used. They felt that their colleagues in surgery would hold them in higher esteem if they didn't seem so wed to strict charts and proscribed doses. In order to counteract the perception that their clinical skill was rendered obsolete by their machines, radiologists were especially vocal in defending their medical art.

4.3 Pastilles and Ammeters

This is not to say that these doctors were not interested in achieving reliable means of measurement. American and British radiologists showed continued interest in this problem, with some doctors hoping to achieve standardization of practice and many more desiring a method for monitoring their own individual technique. But regardless of the role these measurements were meant to play, by the early 1920s, doctors had largely stopped worrying about the reliability of their measuring devices. In the decade leading up to the International Congress in Stockholm, in 1928, when the röntgen was officially adopted as

[376] Enfield, "What the Neighbours Think," 181.

the unit of x-ray quantity, the radiological communities in America and Britain show little concern over the problem of units and of measurement. Each community had settled on a preferred and trusted method, and debates revolved around the physiological action of the rays, and the best course of therapy in particular cases.

In Britain the chemical pastilles of Sabouraud and Noiré had emerged as the favourite method, despite criticisms from physicists and some physicians. Dr. Butcher was one of the doctors who worried about the accuracy of the method. In 1908, he noted how few men were able to distinguish between the colours on the scale. Women, on the other hand, were more accurate, “I suppose from their greater experience in matching colours.”[377] But despite these kinds of concerns, British doctors became more and more confident that the chemical pastille provided stable dosing. Doctors submitting papers in British journals throughout the 1910s and 1920s used the pastille dose unproblematically to communicate their results.[378] This method was lauded as “valuable and reliable,”[379] “exact and correct,”[380] “simple and satisfactory.”[381] In a paper outlining the treatment of TB glands with x-rays, in 1909, Dr. Howard Pirie felt sure that, “the medical man should order

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[377] W. Deane Butcher, "The Measurement of X-Rays: The Standardisation of Röntgen Light " *The Journal of the Röntgen Society* 4(1908): 42.

[378] See for instance W.M Kingsbury, "Cases of Exophthalmic Goitre treated by X-rays," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 6(1913): 158., E.P Cumberbatch, "Case of Keloid treated by X-rays," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 7(1914): 22-23.

[379] H.G Adamson, "Adjourned Discussion on Radio-Therapy," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 14(1921): 22-23.

[380] G. B. Batten, "The Radio-therapy of Superficial Structures," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 14(1921): 9-12.

[381] H.E. Donnithorne and F. E. Baker, "Pastilles and their Colour Measurement," *Archives of Radiology and Electrotherapy* 25(1921): 239.

x-rays as he does drugs."[382] Using a chemical scale where tint B on the Sabouraud scale was enough to produce epilation, Pirie gave the following prescriptions: For ringworm, one epilating dose, for rodent ulcer, one epilating dose every three weeks, for tubercular glands, 1/3 of an epilating dose twice a week. In 1912, when a group of British doctors debated the proper method of treating carcinoma of the breast by x-rays, most of the discussion revolved around the question of how much of a Sabouraud dose should be delivered and with what frequency. For these doctors, the measurements provided by the pastille were assumed to be stable enough to allow them to debate best practice. Of the doctors gathered, only Dr. Morton argued that "there was a need for much more accurate knowledge." [383] But his concern was not with the pastilles in particular. He pointed to a host of other issues that still awaited proper investigation, including research into the quantity of rays absorbed by different tissues in the body.

Pastilles became so trusted in Britain that they were even used as stable measuring devices in investigations into other aspects of radiotherapeutic practice. In a paper in 1915 investigating the effects of different kinds of filters, for instance, pastilles were used to measure the quantity of x-rays passing through each filter.[384] By 1920, Dr. Thurston

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382 Howard Pirie, "The Treatment of Tuberculosis Glands by X-Rays," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 2(1909): 97.

383 E.P Cumberbatch, "Recurrent Carcinoma of the Breast treated by X-rays," *Proceedings of the Royal Society of Medicine: Section of Electrotherapeutics* 6(1913): 38.

384 Agnes F. Savill, "The Sabouraud Pastilles and X-ray filters," *Archives of Radiology and Electrotherapy* 20(1915): 55-56.

Holland felt confident saying that, with a Sabouraud pastille, "it is possible to have a more or less definite knowledge of the exact dosage of x-rays one is giving."[385]

In this same period, the American radiological community likewise settled on a preferred measuring technique. American roentgenologists came to rely on indirect electrical measurements almost exclusively, despite disapproval from American and British physicists that measurements of x-ray properties relying on electrical quantities were "hopeless."[386] American x-ray companies like Waite and Bartlett offered a range of ammeters, voltmeters and milliammeters to doctors to allow them to make these measurements (Figure 26).

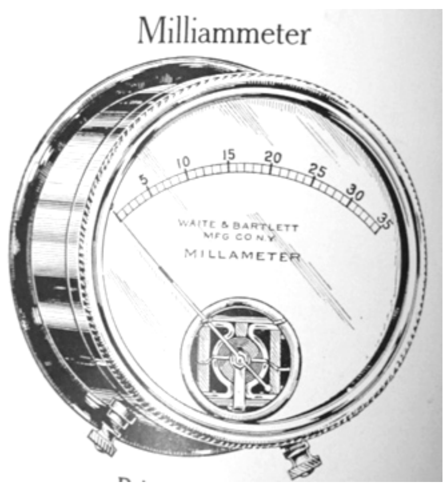

Figure 26: Milliammeter (1910)[387]

[385] C. Thurston Holland, "X-Ray Therapy: An Address to the Southport Medical Society, March 1920," *Archives of Radiology and Electrotherapy* 25(1920): 202.

[386] Walter A. Scoble, "X-Ray Measurement: The Present Position " *The Journal of the Röntgen Society* 3(1907): 99.

[387] Victor Electric Company, *X-Ray and High Frequency Apparatus*: 32.

And a number of American radiologists not concerned with defending their clinical art congratulated themselves on having achieved standardization of practice through the adoption of new equipment. Where the gas tube made it "next to impossible to repeat the dosage in a given case or to duplicate results in a series of cases …" new auto-controlled transformers, the Coolidge tube and reliable milliamperemeters, "made possible the adoption of an absolute standard technique by means of which therapeutic results may be repeated over and over again with almost mathematical accuracy."[388] In the words of the same doctors, the Kienboeck quantimeter was "fussy," and the pastille methods were "not sufficiently reliable," where the indirect measurement of electrical variables was much "more dependable." [389]

One of the leading proponents of this method of measurement was the American dermatologist and roentgenologist George MacKee. Writing in 1919 about the indirect technique, MacKee argued, "It becomes feasible, therefore, to establish a technic that is sufficiently accurate for practical purposes, that will duplicate results and that can be passed from one operator to another."[390] MacKee was so sure that this method eliminated the personal equation, that he gave a set formula for calculating the intensity of unfiltered rays for superficial therapy: [391]

$$\text{intensity} = \frac{\text{current} \times \text{voltage} \times \text{time}}{\text{distance} \times \text{distance}}$$

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[388] J.M. Martin and Chas L. Martin, "Roetgen Ray Treatment of Acne Vulgaris," *American Journal of Roentgenology* 8(1921): 470-71.

[389] Ibid., 472.

[390] MacKee, "Arithmetical Computation of Roentgen Dosage," 603.

[391] Ibid., 604.

Even more practical than this equation were the dose charts the MacKee provided. With a 2mA current supplied to the tube, a 6 inch spark gap and an 8 inch distance between the tube and the skin, a doctor could simply read off of the chart the time required to supply a desired fraction of a skin dose (Figure 27). Dr. Witherbee, a roentgenologist at the Rockefeller Institute, and Dr. Remer, a Roentgenologist at the Vanderbilt Clinic at Columbia University both used MacKee's formula to calculate dose. But they also always used a patient's wrist or back to gauge erythema under different conditions.[392] Despite MacKee's confidence that his method could be used to standardize dosing, at least some of his colleagues continued to assume the clinical relevance of individual reactions to the radiation.

Even more important than this continued strand of resistance to standardization is the simple fact that American roentgenologists, like their British colleagues were clearly content with their measuring techniques. A survey of papers from the early 1920s shows that indirect measurements of the quantity and quality of x-rays by means of electrical variables was widespread. When these American doctors reported details of their therapeutic practice, they consistently included details of these electrical quantities, often giving at least a rough guide to the voltage, current, distance, type of filter used, and length of exposure.[393]

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[392] Wm. D. Witherbee and John Remer, "Unfiltered and Filtered X-Ray Dosage," *American Journal of Roentgenology* 7(1920): 485-92.

[393] See for instance: R.G. Allison, A.H. Beard, and G.A. McKinley, "X-Ry Treatment of Toxic Goitre," *American Journal of Roentgenology* 8(1921): 635-40.; George Holmes, "Some Observations on the treatment of Hyperthyroidism with X-rays," *American Journal of Roentgenology* 8(1921): 730-40.; Lloyd Bryan and Hugh Dormody, "A Preliminary Report on the Effects of Roentgen Rays on Gastric Hyperacidity," *American Journal of Roentgenology* 8(1921): 623-29.; Samuel Stern, "X-Ray Treatment of Hypertrophy of the Prostate," *American Journal of Roentgenology* 8(1921): 292-94.;

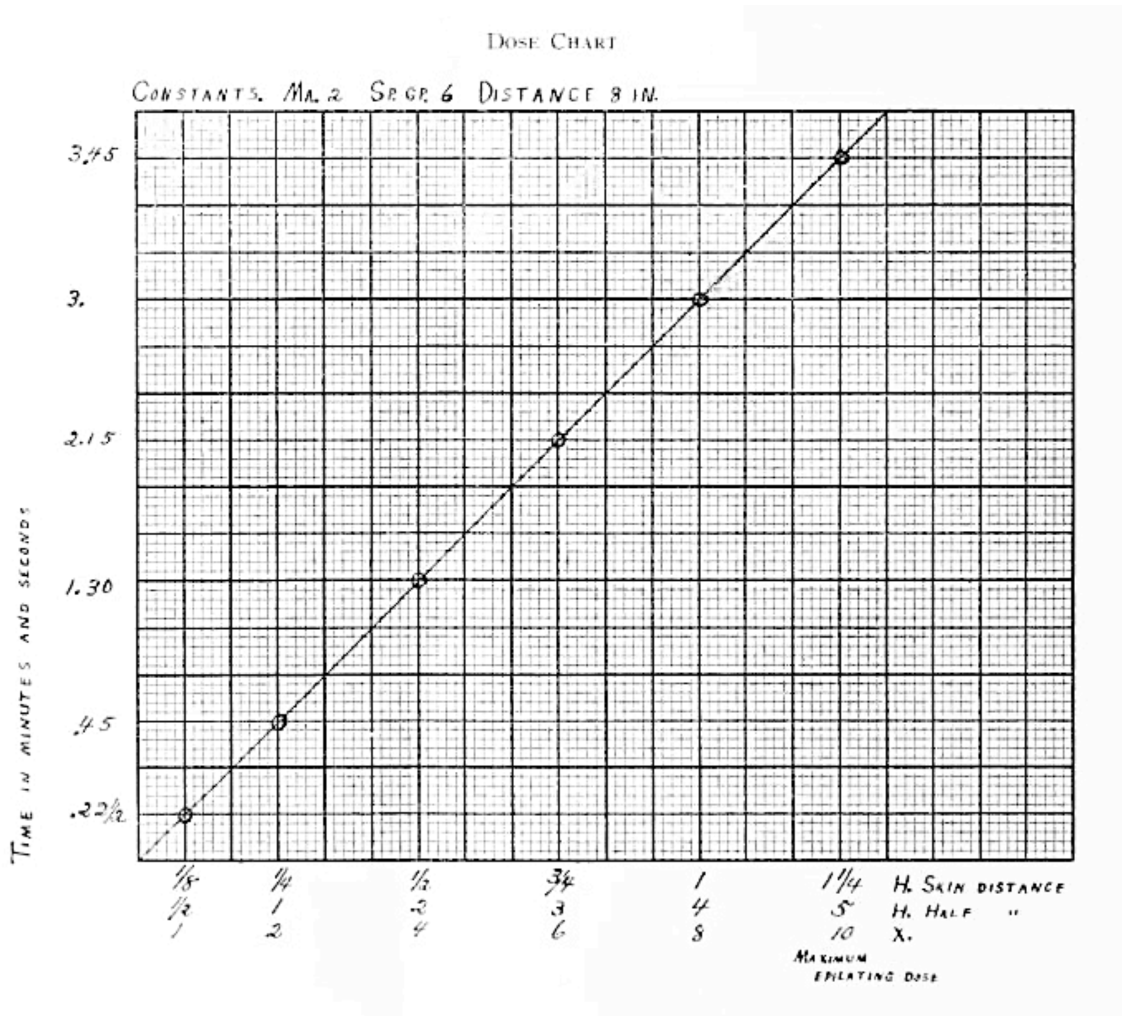

Figure 27: Dose Chart (1919)[394]

George E. Pfahler, "Treatment of Carcinoma of the Thyroid by the Roentgen Ray and Radium," *American Journal of Roentgenology* 9(1922): 20-25.; A.F. Tyler, "Roentgenology of the Thyroid," *American Journal of Roentgenology* 9(1922): 25-29.; D.Y. Keith, "Three Cases of Sarcoma Treated by Radiation," *American Journal of Roentgenology* 9(1922): 31-33.; G.E. Richards, "Possibilities of Roentgen Ray Treatment in Cancer of the Pancreas," *American Journal of Roentgenology* 9(1922): 150-52.

[394] MacKee, "Arithmetical Computation of Roentgen Dosage," 606.

4.4 Physics Insistent

But the physicists were not content. As early as 1906, physicists championed ionization methods as the best means of measuring medical x-rays. These measurements took advantage of the ability of x-rays to ionize air and produce an electric current. Figure 28 shows a schematic of a simple ionization instrument. In one of these devices, a beam of x-rays penetrates a chamber filled with gas, knocking electrons out of some of the molecules, forming positive ions. These electrons then join other molecules to form negative ions. The charged particles are attracted to the charged plates in the circuit, and current will flow from the battery to compensate. A galvanometer placed in the circuit can then be used to measure the current flow. The larger the current, the greater the quantity of x-rays passing through the device.

Röntgen's second paper on x-rays included details of the ability of x-rays to neutralize electrically charged substances and by 1900 a number of investigations by physicists including J.J. Thomson and Ernest Rutherford had shown that x-rays released electrons from solids and gases. These experiments into the process of ionization by x-rays led to the articulation of two empirical puzzles, which Bruce Wheaton has called the paradox of quantity and the paradox of quality. Both puzzles emerged from difficulties in reconciling the dominant model of x-rays with particular experimental observations. By 1900, most physicists assumed that x-rays were impulses in the ether created when electrons abruptly decelerated at the target of an x-ray tube. But the results of ionization experiments performed by Thompson, Rutherford and others were hard to resolve with the model of x-rays as impulses in the ether. These experiments showed that only a small

percentage of the atoms in a volume of gas were turned into ions by a beam of x-rays, in fact far fewer ionization events were observed than would be expected by a spreading wave pulse. This paradox of quantity was joined by a second empirical problem noticed first by William Henry Bragg.

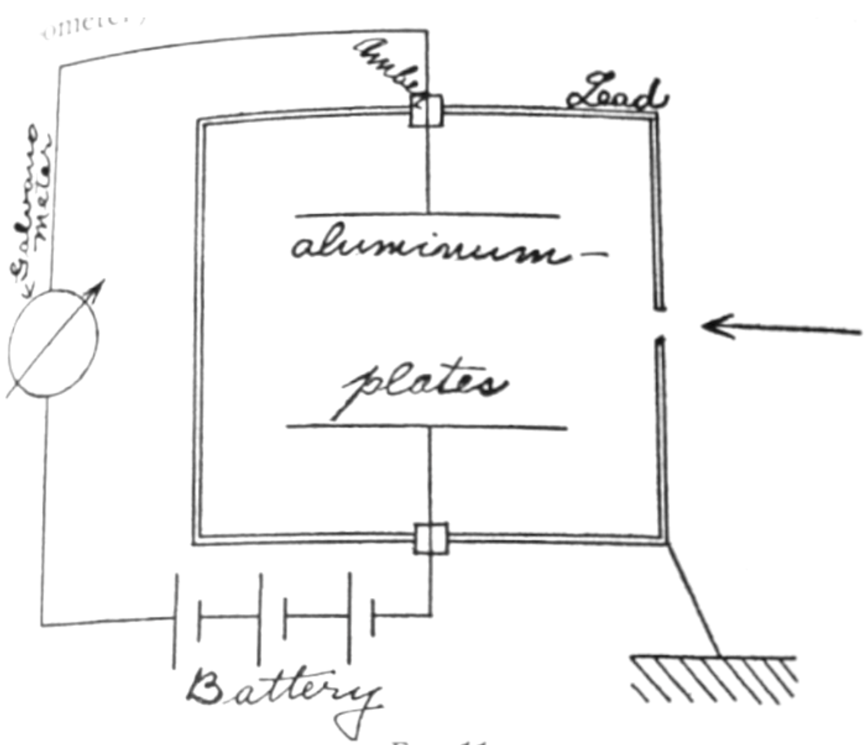

Fig. 11.

Diagram of Ionization Chamber.

Figure 28: Diagram of an Ionisation Chamber[395]

[395] Bachem, *Principles of X-Ray and Radium Dosage*: 67.

Bragg noted that the electron created in the process of ionization had the same energy as the original electron that had hit the target to create the x-ray pulse. The impulse model predicted that the energy in the x-ray pulse would spread out from the target, but instead experiments showed that energy appeared to be wholly localized. Wheaton argues persuasively that these two paradoxes, stemming from experiments with x-rays, constituted a strand of research separate from the theoretical work surrounding optical spectra that contributed to the development of quantum theories in the 1920s.[396]

But these debates, occurring at physics meetings and within the pages of physics journals, did not make it into the medical journals. Even a close follower of the most physical of the English-language radiological journals, *The Journal of the Röntgen Society*, would have had little sense of the research problems occupying physicists. These questions of ontology were not seen to have direct bearing on practical medical problems. Physicists pushed the ionization devices into medicine, not because of their use in these kinds of investigations, but because they themselves had adopted these devices, deeming them to be more precise and objective than the photographic and chemical methods. They wanted their colleagues in medicine to benefit from what they took to be the clear advantages of these instruments.

It should not come as a surprise that these advantages were not clearly evident to the medical community. Historians of physics have illuminated the complex cultural work hiding behind measurements that are often exhibited as self-evident expressions of nature. Precise, quantitative data has not always been awarded the high epistemological status it

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[396] Wheaton, *The Tiger and the Shark*: 76-87.

enjoys today.[397] The desire for precision is a cultural one with roots in late 18th century physics, expressed with great enthusiasm by those engaged in telegraphy research in the 19th century.[398] Simon Schaffer has linked this enthusiasm to commerce: consumers and producers of electricity had to be able to agree on how much electricity was being purchased.[399] Arne Hessenbruch has similarly used an economic model to argue that more exact calibrations in the 1920s in x-ray technology led to greater efficiency, which in turn led to the growth of the x-ray economy. He notes that radiology of the 1920s was, "intent on routinizing and standardizing radiological labor," arguing, "In this, it was merely following a prevalent business logic of the time."[400] But Hessenbruch does not document the resistance to this logic. Quantification has always been political, used to regulate nature and society, and consciously or not, to empower those who become experts in the production of quantitative data.[401] Graeme Gooday has noted, "if privileged significance is attached only to that which is measurable, then those people who cherish what cannot easily be this quantified are likely to experience injustice or at least marginalization."[402]

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[397] Simon Schaffer, "Accurate Measurement is an English Science," in *The Values of Precision*, ed. M. Norton Wise (Princeton: Princeton University Press, 1995), 137.

[398] Bruce J Hunt, "The Ohm is Where the Art Is: British Telegraph Engineers and the Development of Electrical Standards," *Osiris* 9(1994): 48-63.

[399] Simon Schaffer, "Late Victorian metrology and its instrumentation: a manufactury of Ohms," in *Invisible Connections: Instruments, Institutions and Science*, ed. Robert Bud and Susan E. Cozzens (Bellingham, Washington: SPIE Optical Engineering Press, 1992), 23-56.

[400] Arne Hessenbruch, "Calibration and Work in the X-Ray Economy," *Social Studies of Science* 30(2000): 415.

[401] M. Norton Wise, "Introduction," in *The Values of Precision*, ed. M. Norton Wise (Princeton, N.J.: Princeton University Press, 1995), 3-13.

[402] Graeme Gooday, *The Morals of Measurement* (Cambridge: Cambridge University Press, 2004), xvii..

Certainly, in radiology, the voices of the doctors who emphasized idiosyncrasy and medical art were increasingly pushed to the periphery.

Even when it has been valued, achieving precise measurement has not been simple. In his study of 19th century lab physicists and electrical engineers, Gooday has shown that methods of achieving precision were not easily transferable between these disciplines. Part of the reason for this is the inescapable social nature of these measurements. As Norton Wise has argued, "Establishing precision is also a matter of establishing credibility and trust."[403] In radiology, this trust was established as physicists took on leadership roles as safety inspectors and teachers.

Over the three decades following the discovery of x-rays, physicists in the medical world consistently insisted that doctors adopt ionization measurements, but the tone of these entreaties changed over time. What began as a simple articulation of different attitudes became a strong statement of judgment. Doctors were told with increasing insistence that they should adopt the physicists' desire for precision. In 1907, the British physicists Blythswood and Scoble argued at a meeting of the Röntgen Society that ionization methods were the most reliable and promising but in the end left the judgment up to the doctors, stating diplomatically that "It depends whether those employing the x-rays are satisfied with the existing photographic or chemical methods."[404] Almost a decade later, in 1915, the language in an interim report by the British Committee on

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[403] Wise, "Introduction," 3-13. For an in-depth analysis of trust and measurement in late 19th century British electrical science see Gooday, *The Morals of Measurement*.

[404] Blythswood and Scoble, "The Relation between measurements from a focus tube, with a view to determine which are proportional to the intensity of the Röntgen rays," 61.

Standardisation of Dosage was similarly cautious. As you will recall from the previous chapter, the committee was overwhelmingly composed of physicists. Of the 10 members, 7 were physicists and 3 were doctors.[405] Despite this strong representation from the physics community, the language in this report was hesitant. There is no sense of the physicists coming to the table with certain knowledge. The report dealt mainly with existing practice and made only occasional suggestions of changes that could be instituted in order to see a significant improvement in medical practice. The authors of the report wrote, "It is felt that more experimental work is still needed before this can be done successfully."[406] In the previous year, Sydney Russ had traveled to several hospitals to see if x-ray installations operating with the same spark gap were radiating the same quality of x-rays. He found significant differences in the x-ray output of different installations. Yet in reporting these discrepancies his language was apologetic. He told doctors, many of whom would have been relying on spark-gap measurements to determine the quality of the rays produced, that "unfortunately, it does not appear that two apparently similar bulbs which have the same spark gap will give equally hard rays, though this should be put to the test of careful experiment."[407] He did not urge doctors to abandon these measurements completely.

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[405] The members were: Alfred W. Porter (physicist and chair), Robert Knox (doctor), W. Deane Butcher (doctor), W. Duddell (physicist and electrical engineer), N.S. Finzi (doctor), J.H. Gardiner (physicist), W. Hampson (self-taught physicist), G.W.C. Kaye (physicist), C.E.S. Phillips (physicist) and Sydney Russ (physicist). See "Interim Report of the Committee on Standardisation of X-Ray Dosage," *Archives of Radiology and Electrotherapy* 20(1915): 277-83. This report was also published in *The Journal of the Röntgen Society* 11 (1915): 102-110. For more on Hampson see Mansel Davies, "William Hampson (1854-1926): A Note," *The British Journal for the History of Science* (1989): 63-73.

[406] "Interim Report of the Committee on Standardisation of X-Ray Dosage," 277.

[407] Ibid., 278.

By 1917, the tone was beginning to change. According to British physicist George Kaye, almost all x-ray workers now relied on ionization measurements, except for doctors who still relied on the photographic or pastille methods. “It must be admitted that most of these methods, if not all, provide nothing more than the roughest notion of the intensity of a beam of ordinary heterogeneous x-rays.”[408] In 1920, C.E.S. Phillips, another of the prominent British hospital physicists, declared with some force that: “The need of some reliable means of measuring the radiation from an x-ray bulb has now become acute … progress is retarded by the lack of a satisfactory system of measurement.”[409] This condemnation came right at the same time that practice had stabilized in Britain. We have already seen that in the early 1920s British radiologists were content to rely on chemical methods that they considered exact and reliable. But the rhetoric from physicists continued to grow stronger. In 1922, George Kaye, speaking on behalf of his fellow physicists, dismissed the preferences of his medical colleagues, declaring “few of us believe in the future of the pastille as a dose meter.”[410]

American physicists and engineers agreed, criticizing the preferred methods of measurement in medicine, and declaring these methods to be suffering from serious disadvantages. In 1923, Frank Rieber, an engineer from San Francisco, read a paper before the Radiological Society of North America addressing the problem of standardization from an engineering point of view. “The indirect method … is in use widely today but has been

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[408] Kaye, *X-Rays*: 89.

[409] C.E.S Phillips, "A Suggested new method of measuring x-ray dosage," *Archives of Radiology and Electrotherapy* 25(1920): 215.

[410] Kaye, "Radiology and Physics," 41.

determined to be highly inaccurate."[411] He pointed out that brief surges of electricity could lead to errors as high as 50%. He dismissed the unit skin dose, still relied on by doctors to help them quantify individual patient reaction, as "ridiculously unscientific and totally unsatisfactory."[412] Rieber acknowledged that it was impossible for engineers to design a system that could deliver a perfectly stable electrical output, and argued that ionization measurements were therefore required to monitor the quantity of x-rays produced.

Another vocal physicist in the American x-ray community was Albert Bachem. Bachem was a German physicist who is often remembered for his experimental work in the early 1920s in which he reported results in agreement with Einstein's prediction of gravitational redshift.[413] He became faculty at the University of Illinois College of Medicine in the late 1920s, appearing in photos of the graduating class in 1927 and 1931.[414] In his book, *Principles of X-Ray and Radium Dosage* (1923), Bachem provided a detailed analysis and critique of existing methods. In his estimation, the problem with the Kienböck method, and photographic methods in general, was that as energy of x-rays increased the blackening on the photographic paper did not increase in a linear relationship (Figure 29). This meant that photographic methods could only be used to compare beams of the same hardness. "For the calibration or comparison of different Roentgen apparatus the method is

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411 Frank Rieber, "Standardization of Roentgen Output," *Radiology* 1(1923): 154.

412 Ibid.

413 Albert Bachem and Leonhard Grebe, "Über die Einsteinvershiebung im Gravitationsfeld der sonne," *Zeitschrift für Physik* 1(1920).; Helge Kragh, *Quantum Generations* (Princeton: Princeton University Press, 1999), 96.

414 *1927 Graduating Class, University of Illinois College of Medicine*.

entirely unsuitable."[415] He similarly dismissed direct measurements and pastilles, declaring them to be useless in comparing different installations. In his estimation, "The reliability, accuracy, and comparability of these methods leaves much to be desired."[416] Bachem acknowledged that biological scales did have an advantage over physical measurements in that a "closer relation exists between them and the reaction it is desired to produce in human tissues."[417] But he argued that these methods,

> are less exact than physical measurements and … biologic scales are purely arbitrary and subjective … In spite of the many observations which have been made on skin reactions, the statements made by various investigators are widely contradictory with regard to the consistency of the appearance of erythema.[418]

What was, for doctors, a physiological fact guiding individualized therapy, was, for a physicist, a damming failure of a measuring technique. The goal clearly evident to Bachem and his colleagues in physics was to obtain standardized measurements even if the body itself was unstandardisable.

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[415] Bachem, *Principles of X-Ray and Radium Dosage*: 62.

[416] Ibid., 66.

[417] Ibid., 83.

[418] Ibid., 85.

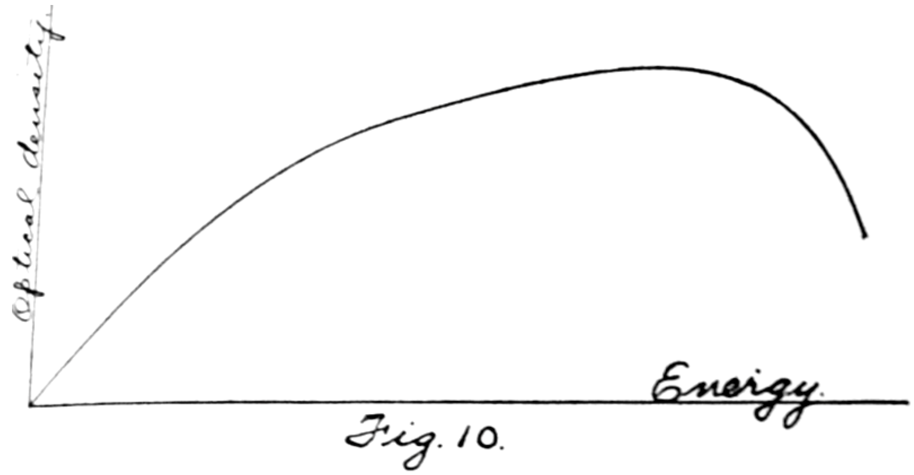

Figure 29: Applied energy vs. optical density of photographic film according to investigations carried out by physicist Albert Bachem[419]

After surveying the alternative methods of measurement, Bachem defended ionization measurements, showing pictures of prototypes of ionization devices that he helped to build designed specifically for medical use [Figure 30]. These devices replaced the galvanometer with a light bulb that flashed when the electroscope discharged. The faster the flash, the higher the intensity of x-rays. The doctor could then simply time the flashes in order to measure the dose of x-rays. But of course it was not quite that simple. Bachem warned that the relation between the time of discharge and erythema exposure varied for different wavelengths, and that the time of discharge also depended on pressure, temperature and humidity, meaning that one would obtain different results in Denver and

[419] Ibid., 61.

Colorado. He advised doctors to routinely use a fixed amount of radium to calibrate their instruments.[420]

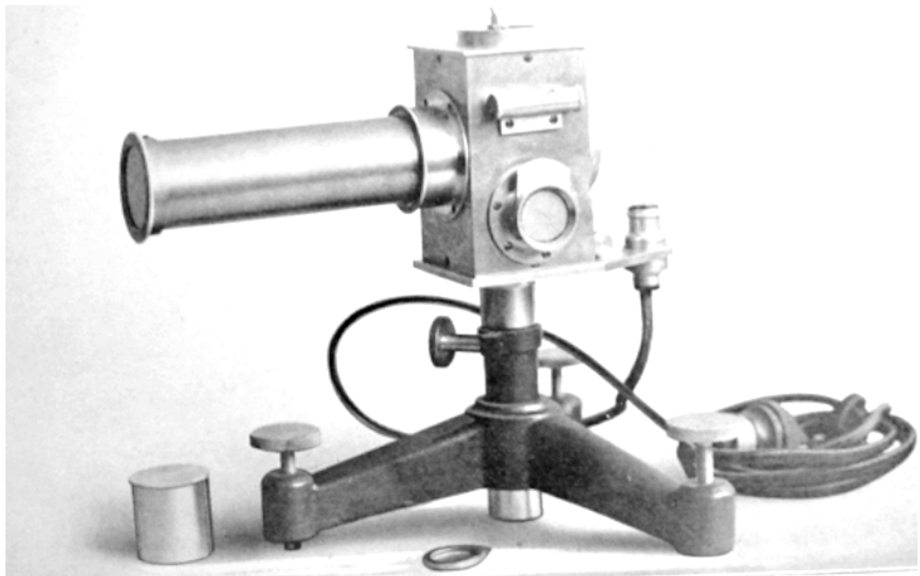

Figure 30: Bachem Ziehn Electroscope for Medical Use[421]

A handful of prominent doctors did feel that it was important to listen to the advice of the physicists, vocally defending the need for doctors and physicists to work more closely together. In 1921, George Holmes, a Roentgenologist at Massachusetts General Hospital and Instructor of Radiology at Harvard Medical School complained that there had been a great failure to increase the number of cures possible with x-rays despite advances in apparatus and physics knowledge. Asking why “the radiotherapist failed to keep step with his co-workers, the physicist and the biologist,” Holmes’ answer was not that these

[420] Ibid., 77-78.

[421] Ibid., 76.

natural sciences were unhelpful to radiologists, but that practicing radiologists had been "overwhelmed by routine work" and lacked the support of student workers.[422] In his Presidential address at the Sixth Annual meeting of the American Roentgen Society in 1921, Dr. Henry Schmitz, argued that the recent progress in radiation therapy "would not have been made had not the sciences of chemistry and physics and medicine worked hand in hand."[423] He felt that "radium therapy and the sister science x-ray therapy are entering upon a new era. Empiricism and uncertainty in the therapeutic application of radiation are being rapidly replaced by scientifically correct methods."[424] He called the "empirical methods" of measuring x-rays in mA-min "obsolete and unscientific" and praised the work of European physicists who had measured absorption curves for different qualities of x-rays. The German researchers, Friedrich, Holfelder and Dessauer and the Americans Glasser, Mayer and Bachem carried out experiments throughout the 1920s using water tanks as model patients and measuring the absorption of x-rays at various depths in order to map out dose distributions.[425] While this research was available to American roentgenologists, summarized in books like Bachem's, it does not appear to have affected clinical practice, at least at this time. There is no evidence of American doctors using these absorption curves to guide their dosage, and the preferred method of measuring dose was one that still relied on the electric variables—current, voltage and time of exposure.

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422 George Holmes, "Some Remarks on the Present Status of X-Ray Therapeutics," *American Journal of Roentgenology* 8(1921): 522-27.

423 Schmitz. Henry, "Relation of the Science of Physics to Radiation Therapy," *American Journal of Roentgenology* 8(1921): 285.

424 Ibid.

425 W.V. Mayneord, *The Physics of X-Ray Therapy* (London: J&A Churchill, 1929), 125-33.

But the calls from physicists were eventually heeded. The Röntgen Society formed the British X-Ray Unit Committee jointly with the Physical Society in February 1923, to look into the problem of an x-ray unit. It was helmed by Sir William Bragg and included prominent hospital physicists including, F.L. Hopwood, C.E.S. Phillips and Sydney Russ.[426] The American x-ray community followed shortly after, constituting the Standardization Committee of the Radiological Society of North America in 1925. Both of these committees were soon coordinating their efforts with an international committee, formed at the first International Congress of Radiology in London.

4.5 Enter the Röntgen

When London hosted the first International Congress of Radiology in 1925—the first congress that the Germans had been invited to since the end of the war—the problem of measurement was a prominent topic of debate. Physicists and doctors from 17 countries including the United States, Canada, France, Germany, Italy and the U.S.S.R. were present.[427] At the joint meeting of the sections of physics and radiology, papers were presented on the theme of "International Units and Standards for X-Ray Work." The first person to speak was Sir William Bragg, who, with great rhetorical flourish, silenced the voices of any practicing physicians who were content with their methods of measurement.

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[426] "Editorial Notes," *Journal of the Röntgen Society* 24, no. 76 (1923): 98.

[427] "The First International Congress of Radiology: Proceedings of the Section of Physics," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 45-157.

> The reason for holding this Congress is so obvious that only a few general words on my part are necessary. You have opening up before you a science of immense importance to humanity, and in order that any science may progress it has always been necessary to make comparisons of results and methods easy … until comparisons can be made, a science can do no more than stumble on its way.[428]

Invoking the oft-quoted Kelvin, "who said that nothing could be said to have made progress until you could put it in arithmetic," Bragg acknowledged that both physicists and doctors would have to work together to determine a standard unit of measure. He divided the problem into three parts. The first was the determination of a unit which was "a matter of physics, pure and simple," the second was the manufacture of apparatus to be used for measurement and the third was the use of this standard apparatus to compare results of treatment, this last step being "entirely a hospital matter."[429] The division of labour envisioned by Bragg gave a role to both doctors and physicists but assumed a homogeneity of goals and values. Despite C.S. Phillips' declaration that "medical men were urgently requiring something by which they could … talk about the radiation they were dealing with,"[430] we have already seen that no such urgent cry was audible from the majority of practicing doctors. But in this setting, those doctors were not the vocal majority. No one rose to champion chemical or photographic methods. And in fact, of the six papers that

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[428] Bragg, "Discussion on International Units and Standards for X-Ray Work," 64.

[429] Ibid., 65.

[430] C.E.S Phillips in "Discussion on International Units and Standards for X-Ray Work," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 101.

followed, five were from French or German researchers. Only one English-speaking x-ray researcher presented a formal paper, and this one voice was that of a physicist, H. Moore.

As the formal papers were read and discussed, the primary debate revolved around the relative merits of two different units based on ionization: a French unit based on the radioactivity of a standard sample of radium, and a German unit that measured the amount of current discharged through ionization under certain atmospheric conditions. The French physician, Dr. Béclère spoke first, defending the unit proposed by Dr. Solomon, an assistant in Röntgentherapy at the department of Hôpital Saint Antoine. This unit had been presented to the Acadèmie des Sciences and the Acadèmie de Médecine de Paris in 1921. It was defined to be:

> The amount of Röntgen rays producing the same ionization as one gram of radium element, distant 2 cm from the ionization chamber, in the same axis of filtration through 0.5 mm of platinum.[431]

For Béclère, the ionometric unit was far superior to any other because the "personal equation does not exist in the observation of the movement of a needle or a shadow on a graduated scale." Giving details of an investigation into the skin unit used in 14 German clinics, he reported that the investigators had found significant differences in the applied dose when it was measured using the ionometric method.[432] Calling this an "extraordinary multiplicity," he argued that the ionometric method was undoubtedly the best method,

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[431] A. Béclère, "On International Standardisation of Measures in Röntgentherapy," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 70.

[432] In the most extreme case, the skin dose in one clinic was measured to be 4 times the strength of the skin dose in another clinic.

"exclusively used by the physicists and on which is based all radioactive science." [433] In particular he defended the French röntgen against the German röntgen, based on work by the German physicist Behnken, which had been adopted by the German Röntgen Society in 1924. Béclère emphasized the difficulties surrounding the German unit, defined as:

> The amount of Röntgen rays irradiating one cubic centimeter of air, at 18° C, under 760 mm mercury, and producing such a conductivity that the saturation current discharges one electrostatic unit, all liberated electrons being used and all action from the ionization chamber walls being eliminated.[434]

Comparatively, "so much is Solomon's unit relatively simple and easy, since it uses a natural substance, identical in London, New York or Berlin." Pointing out that the British Röntgen Society routinely used radium to check the consistency of their apparatus, Béclère asked, "Is not radium therefore rightfully designed for the first and fundamental standardization of these instruments?"[435] He called for an international committee composed of physicians and physicists, acknowledging that, "the physician's point of view will not necessarily be that of the physicists'."[436] In particular,

> Physicists are never satisfied with the precision of their measures and always try to improve it. They, for instance, can measure the heat to the thousandth degree. They are right in doing so, but we are not wrong when we are satisfied in taking the

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[433] Ibid., 69.

[434] Ibid., 70.

[435] Ibid., 71.

[436] Ibid.

> temperature of our febrile patients only to a tenth of a degree. The precision we require is essentially a practical precision.[437]

According to Béclère, the first aim of doctors was to,

> reproduce as exactly as possible the conditions of a treatment given by one of our colleagues on this or on the other side of the Channel or the Atlantic ocean …This modest aim is for us physicians, much more important than the aim of the physicists who are looking towards the determination of the quantity of radiant energy absorbed by each living cell.[438]

The question of international dosage must, therefore, start from "this point of view, the subordination of scientific rigour to the necessities of practice." [439]

The German physicist Behnken spoke next to defend the German unit, acknowledging that dosimeters must be calibrated for different qualities of rays.[440] He argued that the biological dose needed to take into consideration not just the energy applied to the tissue or even the energy absorbed, but also the amount of energy that was "biologically active." He defined this biological dose, B as:

$$B = J\, t\, \mu_g\, s$$

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[437] Ibid., 71-71.

[438] Ibid., 72.

[439] Ibid.

[440] H. Behnken, "The German Unit of X-Radiation," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 72-77.

where J was the intensity of rays, t was the time of exposure and μ_g was the coefficient of absorption of tissue for these rays. The quantity s, measuring the response of the tissues to the rays could never be determined physically. For this, much more biological research was needed. The quantity that *could* be measured was the ionization dose in air but Behnken acknowledged that the relationship between this and the biological dose was still unknown.

The Germans Grebe and Martius spoke next in defense of the German unit. They brought up the problem of the large difference in the skin-erythema dose as measured in röntgens. They had measured 600 r where Duane had measured 1800 r, and Fricke and Glasser 1400 r. The difference was explained to stem from the fact that the American observers were measuring primary and scattered radiation where Grebe and Martius were measuring only the primary radiation.[441]

One of the only speakers at this Congress to mention the methods preferred by practicing physicians was Walter Altschul, Instructor of Röntgenology at the University of Prague. In an overview of current methods of dosimetry, he mentioned that the Sabouraud-Noiré pastille was still employed, and that "practitioners like it very much," but that "the physicists do not use it on account of its inexactness within the range of very hard rays."[442]

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[441] Leonhard Grebe and H. Martius, "Röntgen-Ray Measurments in Absolute Units and Ray-Doses Necessary for Skin-Erythema," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 78-81.

[442] Walter Altschul, "Critical Remarks on the Problem of Dosimetry," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 81-83.

The American physicist, H. Moore was the only physicist not to jump to the defense of ionization measurements. He argued that radiologists did not need to know the total energy of the x-ray beam. Echoing instead the preferences of most American radiologists to measure the electrical variables as an indirect measure of the x-ray properties, he stated that in his opinion, “the problem of measuring x-rays would be solved, so far as the radiologist is concerned, if constant voltage sets were in common use.”[443]

In the discussion that followed, the talk revolved entirely around the need for a standard, and most speakers addressed the suitability of the two röntgen units in particular. Moore’s defense of current American practice was ignored. Sydney Russ, “was glad to see that there was unanimity of opinion as to what ought to be measured.”[444] The group resolved to nominate a small committee to coordinate the formation of a larger international committee to look into the problem. C.E.S. Phillips, the British physicist, ended the discussion on a congratulatory note: “It was well known that the medical people were urgently requiring something by which they could at any rate talk about the radiation they were dealing with.”[445]

Sir William Bragg, Professor Hopwood, Dr. Owen, Mr. C.E.S. Phillips, Professor A.W. Porter and Professor Sydney Russ, members of the British X-Ray Unit Committee were nominated to form an international committee to establish an x-ray unit. This motion was reported favourably in the American periodicals: “This initial group of eminent physicists

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[443] H. Moore, "The Problem of Measuring X-Rays," *The British Journal of Radiology (Rontgen Society Section)* 23(1927): 90-91.

[444] "Discussion on International Units and Standards for X-Ray Work," 98.

[445] Ibid., 101.

and radiologists reasonably assures the probably final solution to one of the most perplexing and difficult problems of the radiotherapeutists throughout the world."[446]

One of the national committees coordinating with this international committee was the Standardization Committee of the Radiological Society of North America, established at a meeting of the Radiological Society of North America in 1925. Like the British committee, this one was to contain a mix of physicists and doctors, in this case no less than three radiologists and three physicists.[447] The first meeting was in March 1926, following the international congress in London. The chairman was Dr. Edwin Ernst, a radiologist from St. Louis. The other doctors were Arthur W. Erskine, from Iowa and William Chamberlain, a radiologist from San Francisco who was also a physicist. The physicists were Otto Glasser, former student of M. Siegbahn, the Swedish Nobel Laureate, and a professor of biophysics at the New York Post-Graduate Medical School, Columbia, and N.E. Dorsey and F. L. Hunt, physicists from the Bureau of Standards.[448] The preliminary report of the committee in 1926 reviewed the methods of measurement available, from chemical colour changes on a pastille to the effects of x-rays on fish eggs: "All of these methods have their individual characteristic advantages, but as a whole they lack uniformity and accuracy, especially when comparing X-ray radiations of different wavelengths."[449] The committee stated that there were "fundamental advantages" to the German röntgen unit, while acknowledging

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[446] Edwin C. Ernst et al., "Preliminary Report of the Committee on Standardization of X-Ray Measurements," *Radiology* 8(1926): 191.

[447] Ibid.

[448] Taylor, "X-Ray Measurements and Protection, 1913-1964," 6.

[449] Ernst et al., "Preliminary Report of the Committee on Standardization of X-Ray Measurements," 192.

the work that still had to be done to take into account different qualities of radiation and to determine the biological effects of different types of radiation.

In the discussion that followed, it was clear that the American radiological community envisioned a central role for the Bureau of Standards in coordinating and implementing research and in supplying new measuring devices. Complaining about not having enough money from Washington, Dr. W.E. Chamberlain, of San Francisco hoped for enough funds,

> to enable the Bureau of Standards to take hold of the situation and put there in that beautiful vault at the Bureau of Standards some ionization chambers and instruments which should be there as experimental exhibits, or permanent instruments to which we can go for a standard, if necessary, a hundred years from now.[450]

The hope was that the Bureau would house at least one primary ionization device, and that secondary, calibrated machines would be delivered to x-ray installations in hospitals and clinics. Dr. Edwin Ernst said, "Such a standardization of an x-ray machine in standard units should preferably be done by physicists or representatives of the Bureau of Standards, who will take into consideration the quality as well as the quantity of X-ray radiation employed."[451] Ernst recommended that the Bureau of Standards employ physicists whose job it would be to monitor x-ray installations "and perhaps issue a certificate to that effect

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[450] Ibid., 196.
[451] Ibid., 194.

to the radiologist in charge of the department."[452] The relationship imagined here, with physicists taking on the job of inspectors, is exactly the role they would come to play in the setting and monitoring of safety standards.

The international committee came to the same conclusion as the American committee and at the Second International Congress in Stockholm in July of 1928, it was announced that the German unit of ionization, the röntgen, would be adopted as the official unit of x-ray measurement. At this time it was officially defined to be:

> the quantity of radiation which, when the secondary electrons are fully utilized and the wall effect of the chamber is avoided, produces in one cubic centimeter of atmospheric air at 0˚C and 76 cm mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured at saturation current.[453]

4.6 Adoption and Resistance

The medical physics communities in both the United States and Britain congratulated themselves, declaring this standardization of measurement an important milestone. Mayneord, the British physicist and the author of *The Physics of X-Ray Therapy* (1929), lauded "this standardization of dosage" as "one of the most important results

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[452] Ibid., 195.

[453] "The Second International Congress of Radiology ", *The British Journal of Radiology (New Series)* 1(1928): 363.

achieved in medical radiology during the last few years."[454] Reaction in the medical community was less exuberant. The physicist, George Kaye, attributed this lukewarm reaction to "a perhaps natural reluctance to give up older methods of measurement."[455] But it was more than simple resistance to new ways of measuring. To start, a number of practical problems remained. Mayneord, for instance, noted the problem of backscatter. He explained this problem with the following thought experiment: If radiologist A measured the dose he planned to administer with an ionization chamber at a distance where the patient would be and radiologist B measured the dose when patient was actually present, by putting the ionization chamber right on the skin, the two would measure very different quantities of x-rays. The 500r that B measured would include backscattered radiation from the patient which would contribute to the total dose received at the surface. Radiologist A, who made his measurement without the patient present, would have measured a smaller intensity of x-rays, and so would end up giving a far higher dose once the patient was actually present.[456] This kind of problem, could of course be solved with standardized technique. But other issues remained. Writing in 1930, the British physician Dr. Walter Levitt complained:

> It is of little value to us to know that 600r has been given if 600r may sometimes produce only a slight effect upon the skin and at other times, that is to say under different conditions, may produce a severe burn. Yet this is precisely what might

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[454] G.W.C Kaye and W. Binks, "An International Comparison of the Röntgen - the Unit of Quantity of X-rays," *The British Journal of Radiology (New Series)* 6(1933): 195-206.

[455] Ibid.

[456] Mayneord, *The Physics of X-Ray Therapy*: 133.

> happen if we were to rely solely upon these physical units, since the biological effect produced by a given number of these units varies greatly, not only with the quality of the ray, but also with the type of apparatus.[457]

A standard unit was agreed upon, but a standard apparatus had yet to be distributed. And it was still unclear how the quality of the rays affected the measurement of quantity. In the discussion that followed Dr. Levitt's paper, Mr. Schall urged doctors not to treat ionization measurements as absolute, cautioning that,

> the art of making the instruments was not yet perfect … When it is used by hospital staffs … working at high pressure, an instrument would not give the same accuracy as when used by the physicist in the peacefulness of his laboratory with everything available where he can check it.[458]

In 1933, The president of the British Institute of Radiology, Dr. Stanley Melville, characterized another important source of the reluctance when he noted that the precision achieved by ionization measurements was perhaps more than was needed by doctors. By this time, confidence in the apparatus was increasing. Tests done by George Kaye had showed that ionization instruments in different countries agreed to within 1 part in 1000. Yet,

> [Melville] did not know whether their medical friends were going to demand from the physicists that the x-ray dosage should be as precise as that. If that were so, the

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[457] Walter Levitt, "Modern Developments in X-Ray Therapeutic Technique," *British Journal of Radiology* (1930): 308.

[458] Ibid., 380.

> physicists would have to demand that the patients themselves should be standardized within the same limits, and he did not know how that would be achieved.[459]

Despite these sources of reluctance, there was a gradual change over to the röntgen as a unit of measurement. In his 1929 *The Physics of X-Ray Therapy,* Mayneord assumed that the only way to measure the quantity of medical x-rays was by röntgens. He did acknowledge that "the supremacy of x-ray measurement by ionization is a very recent phase," and spent time reviewing alternate methods, but only to illuminate their flaws. His one concession to doctors was to give them a rough benchmark for understanding the new röntgen unit, translating 500r as "roughly a pastille dose."[460] Mayneord catalogued the various instruments available at this time and concluded that there was still engineering work to be done to provide a direct-reading ionization device, one that did not, for instance, rely on time measurement.[461]

By 1934, the feeling was that "the international 'r' has been universally accepted and very widely employed in radiotherapeutic clinics for x-ray therapy …"[462] All of the papers on x-ray therapy in the *British Journal of Radiology* in that year communicated their methods in röntgen units.[463] This was quite a change from only a decade before in which

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[459] Kaye and Binks, "An International Comparison of the Röntgen - the Unit of Quantity of X-rays."

[460] Mayneord, *The Physics of X-Ray Therapy*: 82.

[461] Ibid., 81-94.

[462] "A Dosage System for Gamma Ray Therapy," *The British Journal of Radiology* 7(1934): 578.

[463] R. Steward-Harrison, "The Radiation Treatment of Actinomycosis," *British Journal of Radiology* 7(1934): 98-110.; Hugh Davies, "X-Ray Treatment in some conditions of the thyroid and thymus," *British Journal of Radiology* 7(1934): 362-71.; E.M. Haworth, "The Treatment of Syringo-myelia by

the chemical methods were relied on exclusively in Britain. But it should be remembered that the doctors publishing in these journals were a particular, elite group in close contact with physicists in hospitals and through their professional societies. And even while they communicated their dosing in röntgens, each of these papers still referred either to an erythema or unit skin dose, in addition to giving the voltage and filtration used.

Finally, there is evidence of continued resistance to the vision of precision and quantification offered by the physicists. Dr. A.E. Barclay, in 1929, after this triumph of standardization declared in a presidential speech to the British radiological community that in fact, radiology, could never be science:

> Individual peculiarities, susceptibilities, idiosyncracies, are such that we can never be just certain; for there is no standard, no such thing as a perfectly normal person … Lord Kelvin once said that knowledge could only begin when you could express it in figures. You can express science in figures … but art is a different story. … Until you can reduce the problem of life to figures, the practice of medicine, of which radiology is just an aspect, must be an art. Medicine and science may—nay, they must—be collaborators, they may even be partners, but until we know something more of the secrets of life itself, they can never meet on equal terms.[464]

X-rays," *The British Journal of Radiology* 7(1934): 643-53.; R.F Phillips, "The X-ray treatment of some uncommon tumours," *The British Journal of Radiology* 7(1934): 670-84.

[464] A.E. Barclay, "Can Radiology be a Science?," *The British Journal of Radiology* 2(1929): 521.

4.7 Conclusion

In histories of radiology, the adoption of the röntgen unit is often marked as a defining moment in the field. The American medical physicist Lauriston Taylor divides the history of x-rays into the following periods: from 1896 to 1925, 1925-1955 and 1955 to the present. The first was "a period of discovery, application, and a recognized new danger." [465] In the second period came,

> the first organized efforts by medicine to understand, measure and control what it came to recognize as a two-edged sword … It was at the beginning of that second period that the radiological professions throughout the world turned their concerns and energies to control and management of that important medical tool in their possession.[466]

Radiologists before 1925 *were* measuring x-rays, with methods that they trusted, but for Taylor and other physicists, x-rays were not completely controlled until they were characterized by sufficiently precise quantitative measurement.

In the formal decision of the International community to adopt the röntgen and the ionization method of measurement—a method that satisfied the physicists' need for precision—the values of the physicist clearly triumphed. Physicists and doctors may have met initially on equal footing, but as the cultural capital of physicists rose over the first decades of the twentieth century, as radiologists concerned with their professional status

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[465] Taylor, "X-Ray Measurements and Protection, 1913-1964," vi.
[466] Ibid., vii.

within medicine incorporated physics as part of their formal training, solidifying pedagogically the belief in the fundamental nature of physics to their practice, the authority of physics in the radiological community grew.

Where physicists first hesitatingly offered ionization methods as one possible option, they came to vehemently criticize all other methods—even as doctors were content to continue to use chemical methods or measures of electronic variables. Collaboration was not a product of cooperation and equal compromise. Radiologists were slowly trained to think more like physicists.

And yet resistance remained. The body would not be standardized. And so besides illuminating one particular instance of collaboration between disciplines, this story also complicated the question of what was meant by medical science in this period. Charles Hayter has already argued that radium therapy "provides a significant example of the resistance of the medical profession to defer to the ideals of laboratory-based medicine."[467] Treatment by both radium and x-rays forged ahead without waiting for physiological models and explanations for its action. Experience trumped the lab. Here, I've further shown that resistance need not be solely or consciously epistemic; differences in epistemology come wrapped up with differences in values. If early radiology was seen as scientific (a belief clearly not shared by all) then we cannot assume a priori that this meant that the doctors adopted without question the values and commitments of their colleagues in physics.

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[467] Hayter, *An Element of Hope*: 28.

5. Safety Inspectors

Ghastly Impertinence—X-Ray Martyrs—Physics Research and Formal Guidelines—Safety Inspectors—Reassurance and Resistance

"It is a curious thing, but it often happens, that nature appears to resent an intrusion into her secrets, and will sometimes make the intruder pay dearly."

J. H Gardiner, 1916[468]

5.1 Ghastly Impertinence

Physicists and doctors did not agree on measurement but they *did* agree on safety. In one of the strongest examples of shared perspective in this story so far, doctors and physicists understood the danger posed by x-rays in terms of the potential for burns, cancer and electrocution. By focusing on these immediately physiological dangers, medical and non-medical members of the radiological community defined "safety" in such a way as to exclude a number of anxieties that permeated the wider reaction to x-rays. The threat that x-rays posed to personal privacy, the ghostly premonition of death invoked by the skeletal images that circulated in the press and the rays' inappropriate access to the female body were either ignored or ridiculed by x-ray workers.

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468 J.H. Gardiner, "President's Address," *Journal of the Röntgen Society* 12(1916): 3.

This chapter will focus primarily on the reaction of the medical community to the physiological dangers posed by x-rays and the precautionary measures that were eventually adopted by x-ray workers. The first part of this story then, is one of solidarity of vision, of agreement over what counted as a legitimate danger. I argued in the last chapter that the eventual adoption of the röntgen as a the unit of measurement for x-ray quantity demonstrated the influence that physicists had come to enjoy within the radiological community by the end of the 1920s. But I left open the question of *why* they were afforded that authority, or how it was maintained. This chapter will begin to answer these questions. We will see that physicists took on leadership roles in setting safety standards and in enforcing those standards. As safety inspectors, physicists fulfilled a visible and reassuring social function within the radiological community, one that reinforced their special authority and expertise.

When the first x-ray pictures appeared in newspapers and popular magazines there was no hint that this 'new photography' might cause bodily harm, but audiences were disturbed by the new images nonetheless. Joel Howell, Lisa Cartwright and Bettyann Holtzmann Kevles have each emphasized the threat to privacy as primary among the fears surrounding x-rays.[469] Early reports imagined the rays seeing through walls or into luggage as well as peering inside the body.[470] But it was the most private space of all, the interior of the female body that caused the greatest anxiety. Women were often featured in

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[469] See in particular Bettyann Holtzmann Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century* (New Brunswick: Rutgers University Press, 1997), 27-30.; Cartwright, *Screening the body*: 113-25.; Howell, *Technology in the hospital*: 140-50.

[470] Matthew Lavine, "A Cultural History of Radiation and Radioactivity in the United States, 1895-1945" (University of Wisconsin Madison, 2008), 42.

advertisements for x-ray equipment (Figure 31), a potentially shocking sight for viewers as the female body appeared especially vulnerable to the gaze of the "impertinent" rays.[471] In an oft-quoted poem in *Electrical* Review, "These naughty, naughty Roentgen rays" were reproached for their ability to "gaze Thro' clock and gown and even stays!"[472]

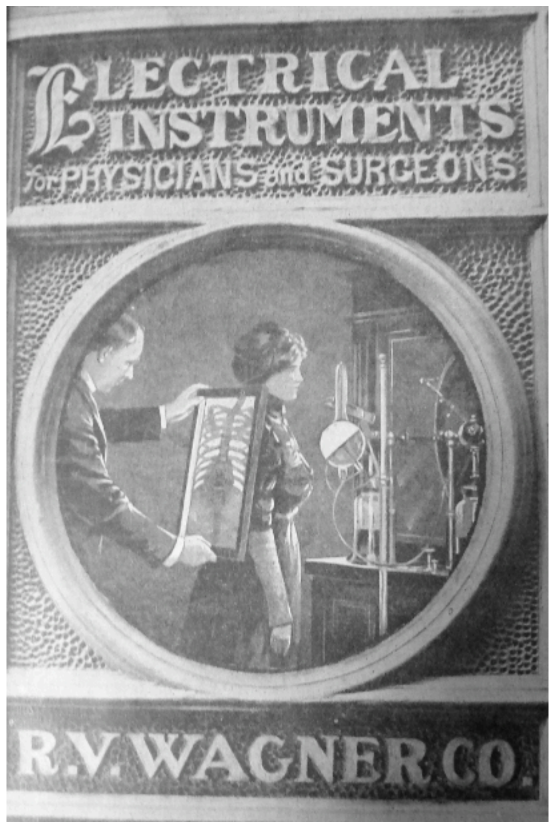

Figure 31: Women were commonly featured at the object of the x-rays' gaze[473]

[471] So described in a poem from 1897 quoted in Kevles, *Naked to the Bone*: 28.

[472] *Electrical Review* April 17, 1896. Quoted in Glasser, *Wilhelm Conrad Rontgen and the Early History of the Roentgen Rays*: 44.

[473] R.V. Wagner Co. *Electrical Instruments for Physicians and Surgeons* (1905).

These kinds of fears were capitalized on by ambitious entrepreneurs selling clothing to protect people from unwanted x-ray exposure. Toronto's *Globe* ran a series of ads in the spring of 1896 for a new type of dress lining, "Textile Buckskin," which, according to the advertisement, provided "perfect protection … against the possibilities of the new photography" (Figure 3).

Figure 32: Advertisements for Textile Buckskin, a dress material impenetrable to x-rays. The picture on the left shows the "ghastly" result of an x-ray photograph and the image on the right shows two women unknowingly being photographed with x-rays. The woman on the right is wearing textile buckskin and her privacy is protected.[474]

This feeling of intrusion remained palpable throughout the first decades of the 20th century. Quoting from a late 19th century Viennese paper which had described the x-ray pictures as "ghastly," the authors of a popular pamphlet on x-rays published in 1927

474 "Textile Buckskin," *The Globe*, Tuesday, March 31 1896.; Saturday, March 14, 1896.

agreed, speaking of "the increasing inquisitiveness of x-rays" which had "laid bare our most intimate anatomical arrangements."[475]

But these threats to privacy and propriety were not the only dangers. The "impertinent" rays were also "gruesome, weird and mysterious."[476] Howell has found expressions of fear and revulsion to be common reactions in literary portrayals of x-rays.[477] The iconography of the skeleton and even of the grim reaper is a common visual theme in the cartoons and advertisements from the era (Figure 33).

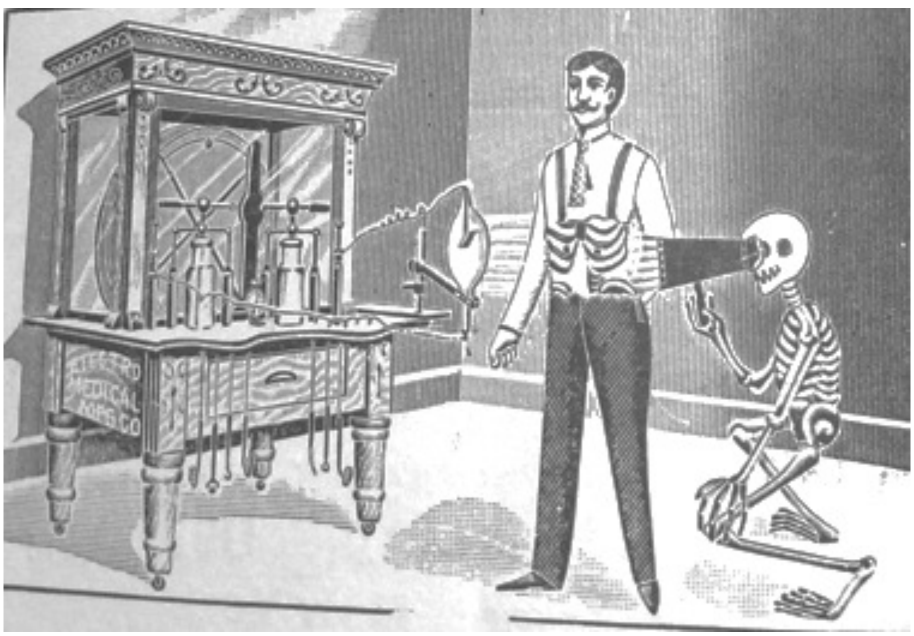

Figure 33: Skeleton Radiologist in an advertisement for Electro-Medical Manufacturing Co. based in Chicago.[478]

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475 V.E. Pullin and W.J. Wiltshire, *X-Rays. Past and Present* (Ernest Bern Ltd., 1927), 140-41.

476 So described in a poem from 1897 quoted in Kevles, *Naked to the Bone*: 28.

477 Howell, *Technology in the hospital*: 140..

478 *The American X-Ray Journal* 7 no.6 (1900), p.x.

Lisa Cartwright has argued that x-rays were so enthralling and so disturbing not because they provided a new mode of representing the body, but because this mode of looking was already quite familiar to late 19th century audiences. These audiences, she argues, would have witnessed skeletons in popular magic shows and spectacles.[479] Alan Grove makes a similar argument, citing late nineteenth century British stage illusions like “Pepper’s Ghost” that showed a man in a coffin dissolving into a skeleton. Grove argues that x-rays were understood in terms of late 19th century ghost stories, with early radiographs appearing to validate the claim that photography would be able to capture spiritual energies invisible to normal sight.[480]

These anxieties were prevalent in the popular press but almost invisible in the medical literature. Yet it was certainly not inevitable that early 20th century doctors would have downplayed the effect of x-rays on their patients’ state of mind and moral character. Keith Wailoo has called turn of the century doctors “moral managers” to draw attention to the medical relevance of overexcitement and injudicious lifestyle habits to diseases of the blood at the turn of the 20th century. Pernicious anemia, for instance, was seen to be caused by imprudent sexual habits and over stimulating work environments as much as chemical constituents of the blood.[481] Doctors as moral managers would be expected to worry about the reactions of their patients to x-rays, especially the reactions of their female patients.

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[479] Cartwright, *Screening the body*: 113.

[480] Grove, "Rontgen's Ghosts: Photography, X-rays and the Victorian Imagination," 141-73.

[481] Wailoo, *Drawing Blood: Technology and Disease Identity in 20th Century America*.

Strong feelings of fear or discomfort, and especially any lowering of sexual inhibitions or immodesty as a result of exposure to the x-rays could affect their health.

But the first doctors to use x-rays did not express these kinds of concerns. When these wider cultural fears appear in turn of the century medical literature they appear as amusing anecdotes. In a letter to the editor in *The Lancet* in 1896, B. Hunter writes in to share "many gems of exquisitely unconscious humour" that she had encountered as a curator at a temporary Roentgen Ray exhibit at a bazaar. Poking fun at the supposed impropriety of the x-ray images Hunter reports,

> Two elderly ladies entered the small room and, solemnly seating themselves, requested me to close and fasten the door. Upon my complying they said they wished 'to see each other's bones,' but I was 'not to expose them below the waist line.'

She goes on to report that "one dignified dame" noted the ability of the rays "to penetrate even one's most intimately protective apparel" and

> One young girl, of the domestic servant class, taking advantage of her opportunity, as she thought, and my sex, asked me in confidence if I would 'look through her young man unbeknown to him while he looked at the pictures to see if he was quite healthy in his internals.'[482]

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[482] B. Hunter, "X-Ray Extravagences," *The Lancet* 148, no. 3817 (1896).}

For Hunter, the potential embarrassment caused by these pictures is merely an amusing anecdote, certainly not a medically relevant fact.

Another report from 1897, took similar tone. In this case, a demonstrator had been exhibiting a fluorescent screen over 6 feet high that was intended to show full body x-rays:

> On an occasion when the apparatus was being inspected by a number of persons it was the means of creating an embarrassment almost beyond the power of words to describe. A lady having incautiously passed in the line of the rays was suddenly displayed on the large screen, and as dress materials are very transparent to the rays her costume, of course, did not count for much in the picture![483]

For Lisa Cartwright, operating in a distinctly Foucauldian framework, the patients' perceptions of the threat to their privacy illuminates the true nature of the x-ray as a tool of regulation, control and surveillance. She argues,

> The press's representation of the x-rays as sexualized spectacle and as a new mode of illicit looking was not at all a misreading of the work of science but a foregrounding of the fact that visual pleasure, sexual desire and the thrill of mortality were not just incidental to the male radiologist's conquest of the inner body.[484]

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483 "A Photographic Exposure," *The Lancet* 149, no. 3842 (1897): 1129; ibid.

484 Cartwright, *Screening the body*: 154.

But as Matthew Lavine has pointed out, very little work has been done to reconcile these cultural narratives with clinical practice and patient experience. It is still unclear how these anxieties manifested themselves in real patient-doctor interactions. Lavine has started to tackle this problem and has found examples of fearful patients in the medical literature, but his examples primarily show patients worried about x-ray burns.[485] It is very rare to find a doctor worrying about a patient's state of mind. In 1896, Henry Chandlee, a Baltimore physician did take time to praise the passivity of a pregnant patient, noting that a 2 hour exposure "was successfully accomplished, with absolute quiet on the part of the patient."[486] His primary concern, however, was that she be absolutely still for the long exposure. Her lack of anxiety was noteworthy only because it ensured a sharp picture.

The only evidence that patients' fears may have impacted clinical practice comes from Joel Howell's close study of the use of x-rays in two North Eastern American hospitals. He has analysed patient records from 1900-1925 and found women were more likely to be examined than men at one of the hospitals and less likely at the other. His best guess as to the gender gap has to do with a number of high-profile accidents involving burnt patients that occurred in one hospital. He speculates that it was these accidents, coupled with the perception that women were more sensitive than men or were perhaps more easily frightened, that contributed to the lower numbers of women receiving x-rays in the

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485 Matthew Lavine, "The Early Clinical X-Ray in the United States: Patient Experiences and Public Perceptions," *Journal of the History of Medicine and Allied Sciences* (2011).

486 Henry Chandlee, "The Roentgen Ray in Diagnosis of Pregnancy," *Southern Journal of Homeopathy* (1896): 4.

hospital that had had the accidents.[487] Even so, the motivating factor in Howell's account is still the physiological dangers posed by the x-rays.

5.2 X-Ray Martyrs

Even though there appears to have been almost universal medical consensus that the pertinent dangers were the immediately physiological, or in a later characterisation, "mischevious"[488] effects of x-ray exposure, adherence to basic safety precautions was wildly inconsistent in the first two decades of the 20th century. For the radiologists who *were* concerned for their safety, manufacturers offered a wide variety of protective devices impregnated with lead. Donning lead aprons, and covering their tubes with lead shields, doctors developed their own rules about the maximum safe exposure for themselves and for their patients. By 1908, Dr. Butcher could declare with confidence that while the early pioneers had "wandered blindly … today none but a knave burns his patients and none but a fool burns himself."[489] With this kind of confidence that the problem had been solved, doctors were not motivated to seek help from physicists or any other party.

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487 Howell, *Technology in the hospital*: 154-57.

488 G.W.C Kaye, *Roentgenology: Its Early History, Some Basic Principles and the Protective Measures* (New York: Paul B. Hoeber Inc., 1928), 68.

489 Butcher, "The Measurement of X-Rays: The Standardisation of Röntgen Light " 70.

The tale of heroic pioneers venturing forth into unknown territory and falling victim to hidden dangers is one that permeates many early histories of this period.[490] In 1925, speaking in front of an international audience of radiologists, the British Minister of Health, Neville Chamberlain, looked back on the early years of radiology with deep admiration for the first x-ray "explorers":

> Ladies and Gentlemen, the path of exploration is always strewn with the wreckage, and often the bones of explorers, and radiology has been no exception in that respect. There have been, in the early stages of experiment, examples of pioneers who in exploring these unknown powers and functions of Nature, have fallen victim to dangers which at that time were unexpected.[491]

This common telling of the story fails to acknowledge the fact that that the physiological dangers associated with x-rays were widely acknowledged within months of Roentgen's announcement. It was easier, perhaps for later commentators to believe that the dangers had been unknown than to believe that the dangers had been ignored.

X-ray burns often appeared weeks after an initial exposure, making it difficult at first to trace their cause. Yet even though the effects of the new rays were "so occult and so long-delayed,"[492] the knowledge that x-rays could cause burns and unwanted epilation came almost immediately after their discovery, and x-rays were linked to cancer within a

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[490] A small pamphlet telling the early story of x-rays published by the Victor X-Ray Corporation speaks of the "pioneers" who had "unknowingly harmed themselves." See Victor X-Ray Corporation, *A Little Journey into the Realms of the X-Ray* (Chicago: Victor X-Ray Corporation, 1926), 23.

[491] "The First International Congress of Radiology: Proceedings of the Section of Physics," 52.

[492] Butcher, "The Future of Electricity in Medicine " 8.

few years.[493] Matthew Lavine has shown that fear of physical trauma developed quickly in the public sphere. By the turn of the century, newspaper accounts of x-rays routinely referred to the rays' ability to "burn" and cause "withering" of tissues.[494]

One of the first published accounts of the potentially harmful side effects of x-rays came in a letter to the editor in *Science* in 1896. The author reported using x-rays to find a bullet in the head of a child who had been shot. This resulted in hair loss which created a bald patch where the x-rays had fallen.[495] In August of 1896, *Electrical Review* published a piece by Herbert Hawkes who had been performing demonstrations of x-rays in department stores in addition to his work in a physics laboratory at Columbia. Hawkes reported sunburn-like symptoms, skin and hair loss as well as changes to his vision.[496] Hearing these kinds of stories, the American inventer Elihu Thomson intentionally exposed his skin to the rays to see what would happen. The result was a reddened finger, that turned swollen and stiff and developed a blister.[497]

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[493] For a detailed examination of the first instances of reported burns and possible links between x-rays and cancer see Daniel Goldberg, "Suffering and Death among Early American Roentgenologists: The Power of Remotely Anatomizing the Living Body in Fin de Siècle America," *Bulletin of the History of Medicine* 85(2011): 1-28.

[494] Lavine, "The Early Clinical X-Ray in the United States: Patient Experiences and Public Perceptions."

[495] Ronald Eisenberg, *Radiology: An illustrated History* (Toronto: Mosby Year Book, 1992), 157. This story is also recounted in Percy Brown, *American Martyrs to Science Through the Roentgen Ray* (Baltimore: Charles C. Thmoas, 1936), 9.

[496] Eisenberg, *Radiology: An illustrated History*: 157.

[497] Brown, *American Martyrs to Science Through the Roentgen Ray*: 11.

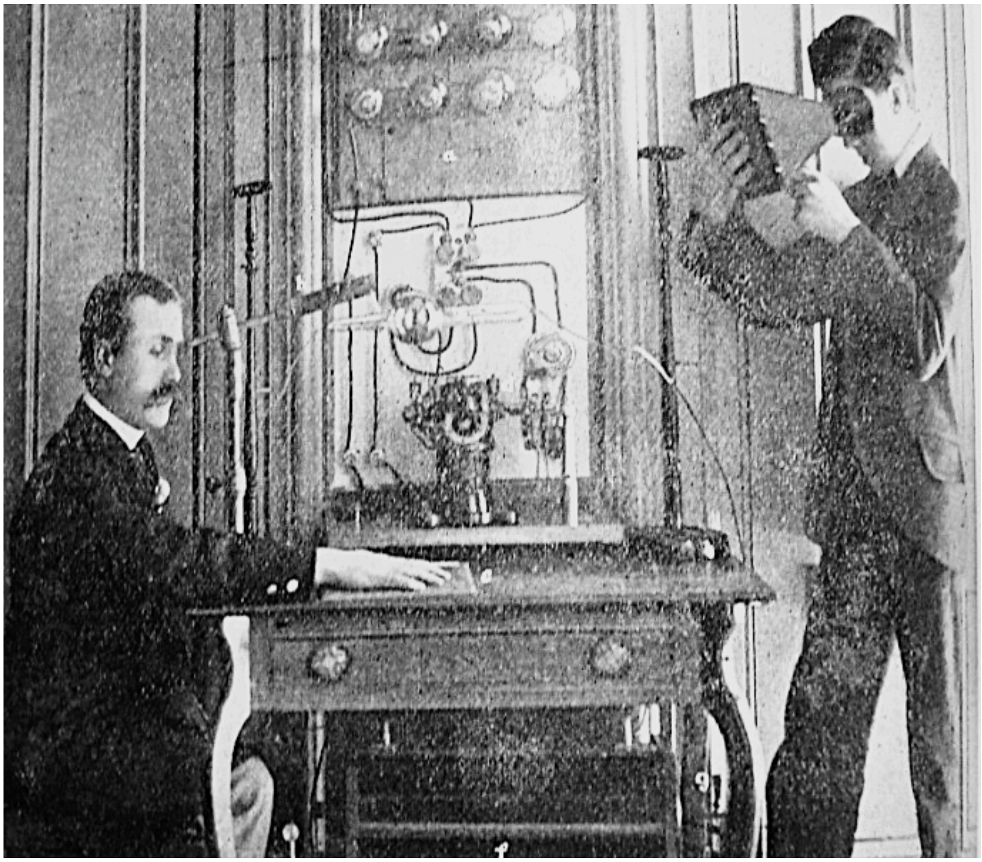

Figure 34: Apparatus in use in the office of Dr. Carl Beck, New York 1897.[498]

The habit that caused the greatest problems for the first x-ray workers was that of judging the output of the tube by placing a hand in between the tube and a fluorescent screen. In Figure 34, showing a photograph from 1897, it is evident that doctors were not at all concerned initially about the radiation being produced by x-ray tubes. Here, the tube is completely unshielded, and the doctors are using their own hands as test objects. The most dangerous of the early innovations was the fluoroscopic screen which allowed physicians to view interior bones and organs immediately rather than waiting for an x-ray

[498] *The American X-Ray Journal* 1 (1897): 82.

plate to be developed. Unfortunately this meant that the physician would be irradiated along with the patient as (s)he leaned in to examine the shadowy pictures on the softly glowing screen. The images in Figure 35 illustrate this practice. In both cases, the gas tube is unshielded and neither patient nor physician appear to have any protection from the rays.

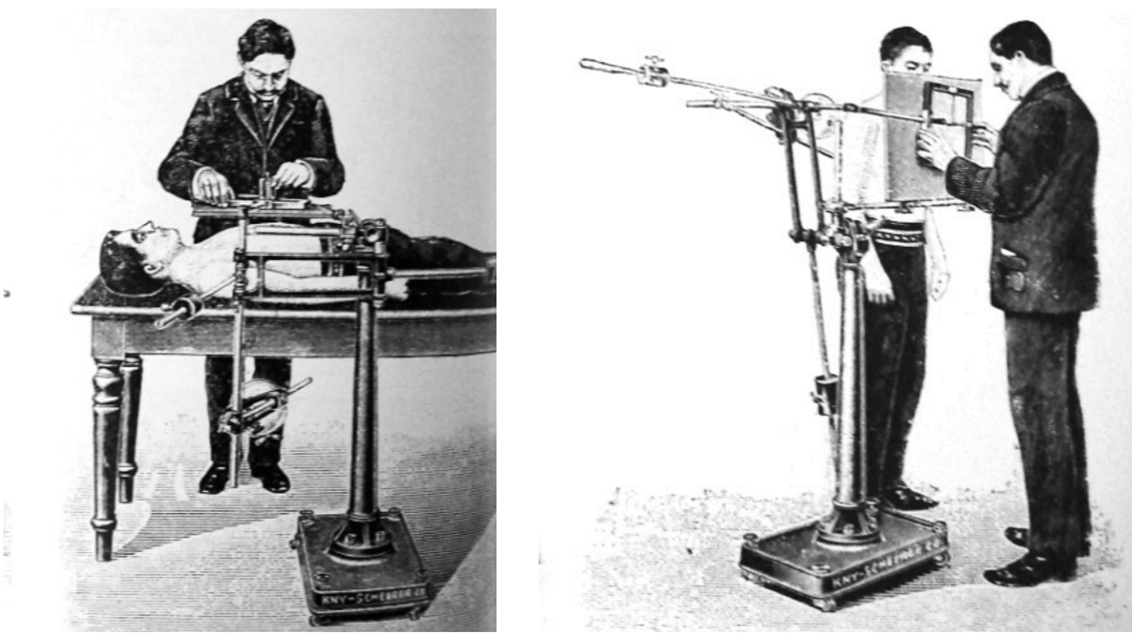

Figure 35: Diagnostic X-Rays c.1905.[499]

But some doctors lost their equanimity early on, writing in to medical journals to give vivid, horrifying accounts of the pain endured from x-ray burns. In a paper read before the British Medical Association in 1904, Dr. Hall Edwards gave the following account:

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499 Kny-Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*.

> Early in 1896 I gave a series of demonstrations lasting four evenings. On each occasion I exposed my hands to the rays for several hours. Some two or three weeks later, I noticed that the skin round the roots of the nails was red and painful. I thought little of this and attributed it to the constant use of the developer containing metol…"

Hall Edwards continued to use his hands to test the penetrating powers of the tubes and soon found,

> the nails of the index finger were the most affected. These began to thicken, and the substance of the nails to degenerate, until they became shapeless masses. Both of these nails have come off on several occasions. About this time, between three and four years back, warty growths first made their appearance." [500]

He began to experience pain "of a dull grinding character." Writing in to the *BMJ* two years later he reported that his state was getting worse.

> I have not experienced a moment's freedom from pain for more than two years, and at times the pain is so severe that I am rendered absolutely incapable of work …[501]

Over the course of treatment, his left forearm was amputated, then four fingers on his right hand had to be removed, and Dr. Hall Edwards died in 1926.

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[500] Quoted in Colwell and Russ, *X-Ray and Radium Injuries: Prevention and Treatment*: 31-32.

[501] Quoted in ibid., 32-33.

Figure 36 shows the same kind of 'x-ray warts' on the hand of Sinclair Tousey, an American roentgenologist. Even more common than these growths was x-ray dermatitis which would cause skin to redden and become rough and scaly. Sometimes ulcers would develop which would cause chronic pain and remain for months. In extreme cases, like that of Dr. Hall Edwards, these ulcers would develop into tumours that would force the amputation of fingers, hands and arms. So many of the early x-ray workers died due to radiation injuries that the German Röntgen Society erected a monument in Hamburg to the x-ray martyrs which contains the names of almost 200 individuals from Western Europe and North America.[502]

Figure 36: X-Ray warts on the hand of American Roentgenologist Sinclair Tousey.[503]

The monument commemorating these 'martyrs' symbolizes a second, and very common telling of this story. Instead of unknowing victims, the early radiologists who

[502] Burrows, *Pioneers and Early Years*: 237.

[503] Tousey, *Medical Electricity, Röntgen Rays and Radium*.

forged ahead with their practice, became martyrs who willingly embraced the danger posed by x-rays and their own suffering. In 1936, the American roentgenologist Percy Brown compiled a small volume with the stories of 27 x-ray workers whom he called "American Martyrs to Science through the Roentgen Ray." Posing a question that continues to occupy historians, he asked why so many of his colleagues continued to work with the rays, often ignoring the available protection equipment, when the dangers were known almost immediately. To answer this question, Brown points to the excitement over the new rays, to the obscurity of work on physiological dangers and confusion over the cause of the burns. He also implicates apparatus makers, arguing that they often advertised the simplicity of their machines, forgoing fussy shields and protective measures in favour of aesthetic minimalism.[504] Cultural historian Rebecca Herzig has noted the importance of the ethos of deliberate suffering to the professional ideal of radiology, arguing that the willingness of these early x-ray experimenters to suffer forged a shared sense of community. She notes that only certain sufferers were afforded martyrdom – the bodies of doctors being privileged in a way that technician's bodies were not.[505] Daniel Goldberg has offered what he calls a complementary explanation to Herzig's, arguing that, "the use of x-ray imaging techniques harnessed the power of seeing the inner body in fin de siècle American culture," and it was this power that made the danger acceptable.[506]

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[504] Brown, *American Martyrs to Science Through the Roentgen Ray*: 18-21.

[505] Herzig, *Suffering for Science*: 85-99.

[506] Goldberg, "Suffering and Death among Early American Roentgenologists: The Power of Remotely Anatomizing the Living Body in Fin de Siècle America," 6.

However, the attitude that I find most common in the first decades of the twentieth century, is not a conscious martyrdom or in fact any acceptance that x-ray work was necessarily dangerous, but instead a widespread sense that the dangers of x-rays had been conquered. By 1910, doctors were aware of the harmful effects of x-rays and felt that they had these unwanted side-effects under control, using basic lead shielding, rules governing maximum exposure times, and remedies for burns when they did occur.

In fact, the primary fear surrounding x-ray equipment was often not the dangers due to x-ray exposure, but the danger of electrocution from the millions of volts needed to power the x-ray tubes. This high voltage was often measured via a spark gap, and the electrical discharges produced were loud, impressive and, for many doctors, rather worrying [Figure 26]. In a 1905 pamphlet, the manufacturing company Wagner addressed the concerns of doctors who were afraid of using their electrical equipment. Wagner assured doctors that their static machines produced a very small current and were therefore quite safe. [507]

But the static machines sold by Wagner became less and less popular, and the danger of electrocution from the high voltage current drawn by x-ray equipment remained acute. Motivated by the deaths of one physician and two patients from accidental contact with the current, the American Roentgenologist George Pfahler offered a new device in 1920 to protect against electrocution. If the patient came within sparking distance of any of the live wires in the room, the air could become a conductor and a large current could flow into the patient. Pfahler's protective device kept the patient well away from the

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[507] R. V. Wagner Company, *Catalogue of Electrical Instruments for Physicians and Surgeons*: 61.

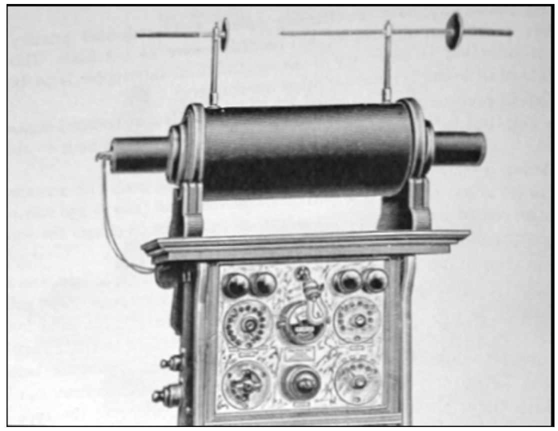

Figure 37: Spark Coil c.1908.[508]

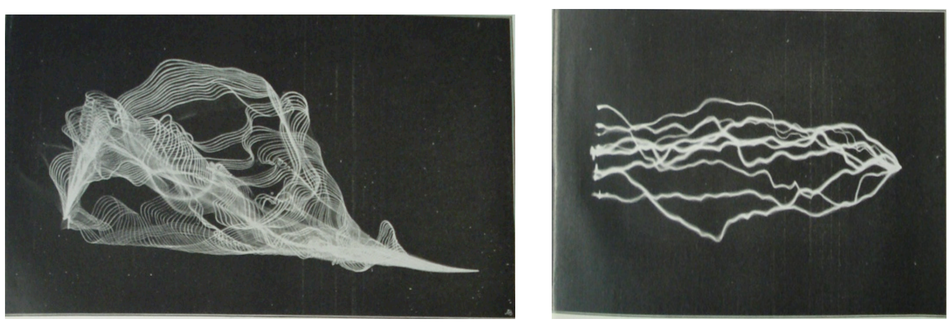

Figure 38: Discharge produced by coil shown in Figure 25.[509]

conducting elements of the x-ray apparatus. [510] The worry over electric shocks and electrocution remained of primary concern throughout the 1920s and 30s. X-Ray apparatus

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[508] K. Schall, *Electro-Medical Instruments and their Management: Illustrated Price List of Electro-Medical Apparatus*, 10th ed. (London: K. Schall, 1908), 168.

[509] Ibid., 169.

[510] George E. Pfahler, "A New Device for increasing the Protection of Both the Patient and the Roentgenologist," *American Journal of Roentgenology* 8(1920): 239.

were advertised as being "safe" because they were electrically safe and shock proof [Figure 39].

But doctors were also concerned about overexposure to the x-rays, and many x-ray manufacturers began to offer protective equipment designed to shield against unnecessary radiation. The Kny Sheerer Company declared in 1905, that "it has become a question of vital interest as to how to protect the operator … The loss of a hand or an arm is not a thing to be contemplated with equanimity."[511] The company offered protective cabinets for the operator to stand in with lead glass windows that would allow the doctor to observe the patient during an x-ray procedure. Figure 40 shows the same kind of protective screen offered by the New York Company Waite and Bartlett. If the operator needed to approach the patient during an exam or therapeutic session, (s)he could don a full body suit like the one shown in Figure 41. Kny Sheerer additionally offered lead glass and sheets of lead foil covered in rubber so that physicians could construct their own cabinets and aprons to their desired specifications. Many companies also offered lead shielding to cover x-ray tubes, or, as in Figure 42, x-ray tubes made almost entirely out of lead glass to ensure that the rays were not leaving the tube in all directions.

[511] Kny-Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*.

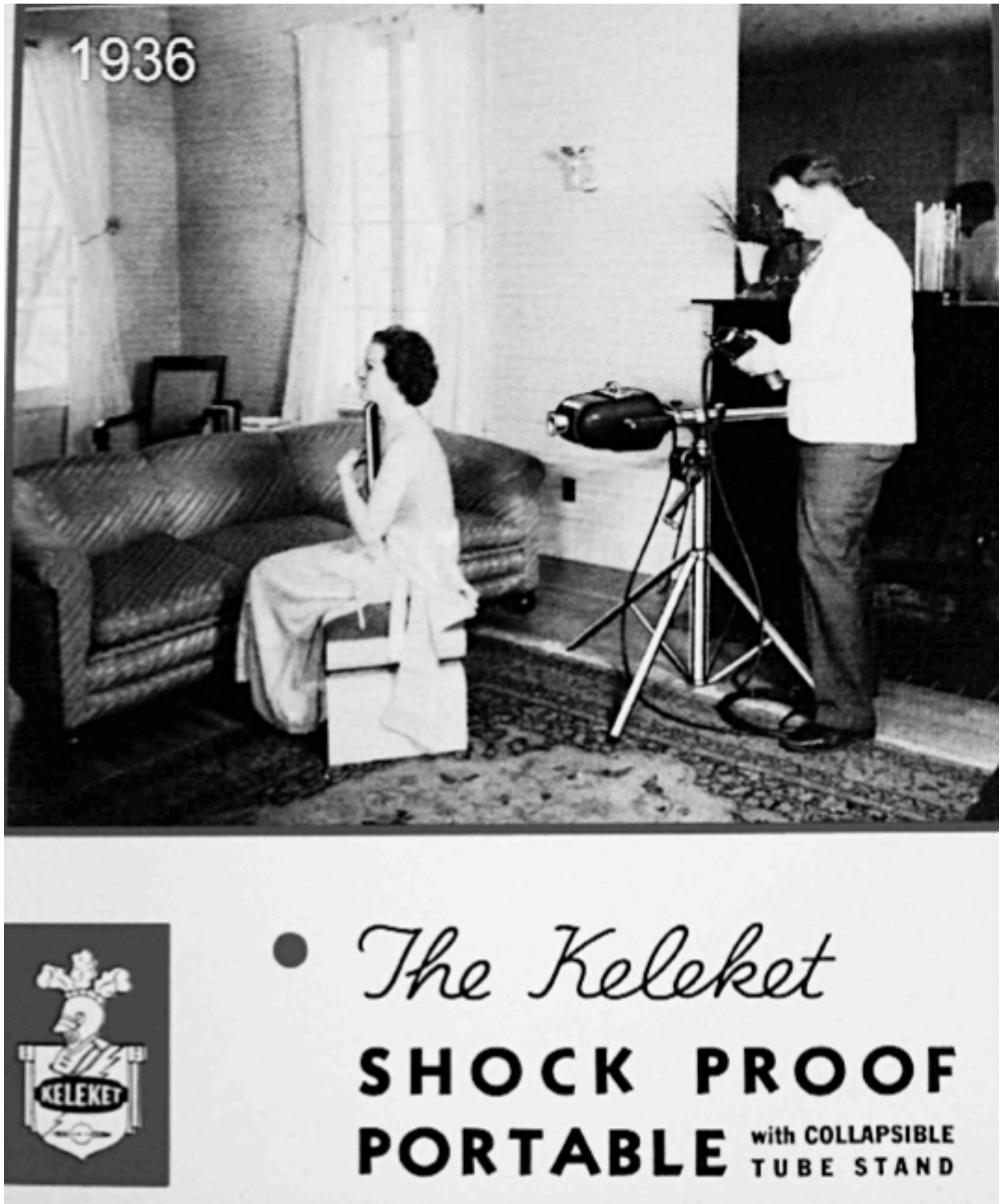

Figure 39: Advertising from the late 1930s appealing to a continued concern over electrocution and shocks from x-ray apparatus.[512]

512 The Kelley-Koett Manufacturing The Kelley-Koet Manufacturing Company, *The Keleket Shock Proof Portable with Collapsible Tube Stand* (Covington, Kentucky1936).

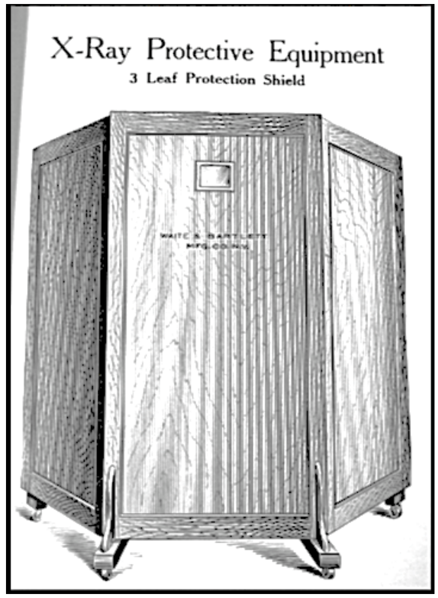

Figure 40: 3 Leaf Protection Shield (1910).[513]

Figure 41: Full Protective Suit c.1905. Rubber covered lead foil including spectacles made of leaded glass.[514]

[513] Waite and Bartlett Manufacturing Company, *X-Ray and High Frequency Apparatus*: 25.

Figure 42: Safety X-Ray Tube (1910).[515]

These measures were designed to protect the operator who would receive multiple exposures over the course of weeks and years in a busy x-ray department. But doctors were equally worried about the possibility of needlessly harming a patient. Reports of lawsuits showed up from time to time in radiological journals. In 1906, *The Journal of the Röntgen Society* reported that Dr. Holzknecht, a famous and well respected pioneer of x-ray therapeutics and the director of the X-ray Department of the Vienna General Hospital had had to pay 1450 pounds in damages for burns suffered to a patient. The incident had

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[514] Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*.

[515] Company, *X-Ray and High Frequency Apparatus*: 21.

occurred in 1902, and the patient had testified in court that he had not been aware of the risks of x-ray exposure.[516] Doctors were warned by manufacturing companies that " it is possible to inflict injury with the x-ray …" But the message from those companies was that the machines could be operated safely if the right procedures were followed. The Wagner Company gave the following instructions in 1905:

> The length of exposure must be governed according to the volume of current passing through the tube, the distance of the tube from the patient, and the nature of the current employed. When these factors are taken into consideration there is not as much danger from the use of x-rays as there is in prescribing strychnine and many of our most efficient remedies.[517]

In the event that an x-ray burn did occur, The Kny-Sheerer company kept a salve in stock and gave advice on how to treat the three levels of burn that might develop. For simple dermatitis, they recommended their "Burrows Solution," be applied to the affected area. For 2nd degree burns, they recommended a gauze dressing plus xeroform powder and then xeroform-lanolin ointment, and for necrotic or third degree burns they instructed that the tissue be removed completely and the resulting wound treated.[518]

Doctors echoed the sentiment prevalent among manufacturers that x-ray burns were avoidable and treatable. In 1906, John Hall Edwards, president of the British Electro-

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[516] "Notes," 22.

[517] Company, *Catalogue of Electrical Instruments for Physicians and Surgeons*: 61.

[518] Kny-Sheerer Company, *Illustrated and Descriptive Catalogue: Roentgen X-Ray Apparatus and Accessories*.

Therapeutic Society and Officer in charge of the X-Ray and Light Department at the General Hospital in Birmingham, published a paper on X-Ray Dermatitis. He purposefully left out questions of the cause of dermatitis, noting

> We have reason for believing that, what we call x-rays, are not a beam of rays exhibiting measurable properties, but a bundle of rays, each capable of exhibiting immeasurable and diverse properties.[519]

An exact determination of the nature of x-rays was seen to be unhelpful, instead the doctor needed to understand that the severity of the symptoms depended on the nearness of the tube, the length and frequency of exposure, the general condition of the tube (whether it was hard or soft), the health of the patient, the protection of the parts not being x-rayed, and previous applications of x-rays.[520] In general, Hall Edwards advised doctors never to exceed a 3 minute exposure. Soft tubes, he cautioned, were especially dangerous because of their likelihood to produce burns and should never be used to radiograph a part requiring more than a few seconds exposure. Noting how hard it was to set out exact rules when conditions varied in each setting, he gave general guidelines for different parts of the body: 10 seconds for hands and up to 3 minutes for a chest x-ray. He also entreated doctors to make use of lead-glass and lead aprons sharing the fact that he personally suffered from a case of chronic dermatitis that was worsening. But his overall tone was extremely optimistic. He felt that if these guidelines were followed,

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519 John Hall-Edwards, "X-Ray Dermatitis, Its Prevention and Treatment," *The Practitioner: A Medical Journal*, no. 114-126 (1906): 115.

520 Ibid., 116.

> Acute x-ray dermatitis is absolutely avoidable, when the rays are used only for the purpose of diagnosis, and should it occur, the operator has only himself to blame.[521]

He was confident that in the near future acute x-ray burns would be "unknown." In his mind, the most important preventative measure was training for the physicians:

> It is now pretty generally recognized that a special training is necessary in order to fit a man for the successful carrying out of x-ray work, and the sooner that a knowledge of electrotherapeutics is made a necessary part of medical training the better for the public and profession alike.[522]

Over the next decade, Hall Edwards' attitude of optimism seemed well founded. In 1914, Dr. W.J. Dodd of Boston presented a paper on "Acute Roentgen Ray Dermatitis," but apologized, "for mentioning such a subject before a gathering of Roentgenologists, some of whom have probably not seen such a condition, certainly not in their own practice." He felt justified returning to this topic, even though "we all know that there is absolutely no danger when the trained Roentgenologist does this work,"[523] because of the new Coolidge tubes. Dodd argued that these new tubes had "added another source of danger if placed in the hands of those ignorant of [their] power."[524] Dodd was confident that even if a reaction did occur, it could easily be treated. He offered an "exceedingly simple and absolutely

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521 Ibid., 119.

522 Ibid., 121.

523 W.J. Dodd, "Treatment of Acute Roentgen Ray Dermititis," *The American Journal of Roentgenology* 1(1914): 430.

524 Ibid.

efficacious remedy" consisting of a salve made of zinc oxide, phenol, glycerine, and aquae calcis.[525]

By the end of the second decade of the 20th century, doctors felt sure that their protective measures were effective and there were even some physicians who continued to argue that adverse reactions to x-rays were actually due to some other cause. In 1918, George Pfahler maintained that the side effects of x-ray treatment were mainly due to the inhalation of gases.[526] In the same year, Eugene Caldwell acknowledged that roentgen ray alopecia and dermatitis were possible with diagnostic x-rays, but was happy to report zero "accidents," using a sheet of aluminum as a "protective ray filter."[527] In the pages of *The American Journal of Roentgenology*, the French doctor, T.H. Nogier, advocated complete adherence to safety procedures, using gloves, aprons and goggles and avoiding all direct radiation:

> When combining all these procedures and using them in a systematic way, one may not avoid all the dangers, but one may reduce them to an infinitesimal proportion.[528]

The physics of x-rays appeared to have little to offer this problem. Doctors investigating the physiology of x-ray action on the body would sometimes appeal to

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[525] Ibid., 431.

[526] George E. Pfahler, "The Cause and Prevention of the Constitutional Effects Associated with the Massive Doses of Deep Roentgenotherapy " *American Journal of Roentgenology* 5(1918): 5-9.

[527] Eugene Caldwell, "Skiagraphy of the Accesory Sinuses of the Nose," *American Journal of Roentgenology* 5(1918): 570.

[528] Peer Lund, "Procédés de Protection," *American Journal of Roentgenology* 5(1918): 448.

physics, but only in very general terms. In 1918, Dr. Béclère appealed to physics to make the argument that the physiological action of x-rays and radium should be the same.

> Since we know today that the roentgen rays and the gamma rays of radium are of the same nature, and in proportion to their penetrative power have practically the same properties, their only real difference consisting in an inequality of wave-length – therefore a type of action which is demonstrated to occur with one should also exist with the other.[529]

But with the lead shields in place doctors felt that they were well protected. And the question of patient protection, like therapy, was left up to the discretion of the doctor, who had to take into account the general health of the patient, his or her prior exposure to the rays as well as the state of the tube and the length of exposure.

5.3 Physics Research and Formal Guidelines

With this sense that doctors and patients were adequately protected against x-rays, it is not surprising that there was no sustained push in the early 20th century for a formal set of safety rules to guide radiologists. When those rules did appear, they were inspired first by physics research into the penetrability of actual protective materials and then by the deaths of a number of prominent radiologists who developed blood diseases after exposure to the more powerful x-rays generated by new Coolidge tubes. Without

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[529] Béclère, "The "Penetrating-Irradiations-Sickness"," 502.

consensus as to the physiological effects of the rays, the voices of physicists were prominent as formal guidelines were debated and adopted by the American and British radiological communities.

The first committee to look into the harmful effects of x-rays was formed by the Röntgen Society in April of 1898, but that committee did not produce a formal report.[530] The problem of safety did not reappear again in the British community until 1915. In June of that year, Sidney Russ, the physicist at Middlesex Hospital, read a paper "On Protective Devices for X-Ray Operators," at the Röntgen Society. He reported that he had tested samples of lead rubber and lead glass from three different (unnamed) manufacturers and found a wide variation in the transmission of x-rays through these samples. In particular, he warned that it was useless to try to judge the protective values of the materials by their thicknesses, when what mattered was their density and lead content. He also warned that a pastille was not sensitive enough to reliably alert a doctor to the presence of stray radiation in an x-ray room. Russ' report sparked concern among those present at the meeting and a resolution was passed for the Society to come up with a set of universal guidelines for the protection of x-ray operators.[531] This was taken up quite promptly and the first set of formal rules was published in the *Journal of the Röntgen Society* in January of 1916.

These first guidelines were simple, urging doctors to make use of available protective devices such as lead screens, lead gloves and aprons and under no

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[530] The May 1898 edition of the *Lancet* reports that a committee on the injurious effects of x-rays had been struck by the Röntgen Society. See also F.G. Spear, "The British X-Ray and Radium Protection Committee," *The British Journal of Radiology* 26(1953): 553.

[531] Sidney Russ, "On Protective Devices for X-Ray Operators," *Journal of the Röntgen Society* 11(1915): 110-13.

circumstances to use their own hands to test the penetration of the rays. Russ' concern about the variability of these protective devices was officially reproduced in the recommendations, which read: "Operators should be warned that shields obtainable commercially are often ineffective and tests of their opacity should be made."[532] At this stage, though, no quantitative recommendations were made about *what* level of opacity would be considered safe. In 1917, British physicist George Kaye offered the following quantitative recommendations: "In all ordinary circumstances the following thicknesses of protective screen may be considered adequate for the operator: Lead: 2mm; lead-impregnated rubber: 8mm; lead glass: 10-20 mm."[533] Kaye did not, however, address the problem of variability in the lead content of manufactured lead rubber and glass. That problem remained unsolved until later guidelines were published that specified protective values in terms of the lead equivalence of the protective materials rather than their thickness.

In the United States, the researchers interested in the problem of safety were also primarily physicists. One of these was W. S. Gorton, a physicist who had received his PhD from Johns Hopkins in 1914. Gorton had worked as a physicist at the Brady Urological Institute at Johns Hopkins Hospital for 3 years before becoming an associate physicist at the Bureau of Standards.[534] While employed at the Bureau of Standards, he undertook research testing the effectiveness of commercially available x-ray protective devices. He

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[532] Reproduced in Kaye, *X-Rays*: Appendix V.

[533] Ibid., 169.

[534] American Telephone and Telegraph Company, "List of Contributers," *The Bell System Technical Journal* 11(1932): 323.

purchased 12 pieces of lead glass from "representative dealers" and found that many of them afforded very little protection, and two of the samples were no more protective than ordinary plate glass. On behalf of the Bureau of Standards, Gorton went on to correspond and collaborate with various manufacturers who were able to significantly improve their lead glass and lead-impregnated rubber materials.[535]

During WWI and immediately afterwards, this kind of research sparked renewed interest in the problem of safety. By 1918, lectures on safety had become part of the training on x-rays for students in the medical corps of the United States Army[536] and the Roentgen Ray Department of the Army Medical school in Washington D.C. also reported studies on the protective value of the lead glass, gloves and aprons supplied for use in the army.[537]

With research casting doubt on the effectiveness of available protective devices, the American Roentgen Ray Society followed the British lead and formed a new Safety Committee. The committee was convened in 1920 at their annual conference, just a few years after the publication of the first British safety guidelines. This American committee consisted of one physicist, Prof. J.S. Shearer of Cornell, one doctor, P.M. Hickey, and one

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[535] W.S. Gorton, "Roentgen Ray Protective Materials," *American Journal of Roentgenology* 5(1918). He thanks in particular the cooperation of the Corning Glass Works, Pittsburgh Plate Glass Co., The B.F. Goodrich Co., Manhattan Rubber Mfg. Co., and the Price Electric Company.

[536] Ashbury, "Annual Report of Roentgen Ray Department Army Medical School, Washington, D.C., 1917-1918."

[537] Ibid.

representative of industry, Dr. W.D. Coolidge.[538] Their work was stalled, however, by the death of the chairman, Prof. Shearer,[539] and when the committee did make recommendations in 1922, they followed those already established by the newly created British X-Ray and Radium Protection Committee.[540]

Physics research demonstrating the variability of protective materials had caused some renewed interest in the problem of protection, but it was the deaths of a number of radiologists in the early 1920s that gave the problem of x-ray safety a sense of urgency. These deaths not only shook the radiological community in Britain, but as Sidney Russ later remembered, "there was a degree of public alarm which … threatened to restrict the use of X rays among the public generally."[541] In particular, it was the death of Dr. Ironside Bruce, a radiologist at Charing Cross Hospital and one of the lecturers for the new Cambridge diploma of radiology, that stirred the community of radiologists in London to action. Bruce died on March 21, 1921, after 2 months of illness. He had been diagnosed with aplastic anemia, a blood disorder attributed to overexposure to the high-voltage x-rays produced by the new, and much more powerful Coolidge tubes. Obituaries and testimonials poured in to the medical journals and newspapers. Bruce was remembered for his "genial good nature, ability as a teacher, and self-sacrifice as a teacher,"[542] as well as the "exceedingly

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538 "New Committees," *The American Journal of Roentgenology* 8(1921): 204.

539 George E. Pfahler, "Protection in Radiology," *American Journal of Roentgenology* 9(1922): 803-06.

540 Spear, "The British X-Ray and Radium Protection Committee," 554.

541 Sidney Russ, "A Personal Retrospect," *The British Journal of Radiology* 26(1953): 554.

542 "The Late Dr. W. Ironside Bruce," *British Medical Journal* 2(1921): 514.

complete and efficient x-ray department" under his care.[543] It was reported that King George V himself had sent a note of condolence to Charing Cross Hospital.[544] The story was taken up in the popular press. The medical correspondent to *The Times* blamed the power of the new x-ray tubes, telling readers that the powerful rays had caused the destruction of Dr. Bruce's blood. And this was not an isolated incident. Sounding the alarm, this author warned that several radium workers had recently died of this condition, as had an x-ray worker in Italy.[545] The death of Dr. Adolphe Leray, a French radiologist was reported the same week.[546] Readers of *The Times* were warned of the new dangers posed by x-ray therapy now that the penetrating power of the tubes had increased more quickly than the efficacy of the protective devices.[547]

Dr. Bruce's death prompted some intense introspection on the part of radiologists. Sir Dawson Williams, the editor of the *British Medical Journal* wrote that "the position is very unsatisfactory; the precautions suggested are confessions of ignorance." While Williams felt that "inquiry into the physics of both radium and the x-rays has gone a long way," physiological research into the action of radiation on tissues still had very few answers. He called for renewed research into the "basic principles" of x-rays, physical,

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543 "Obituary: William Ironside Bruce, MD," *British Medical Journal* 2(1921): 481.

544 "The Late Dr. W. Ironside Bruce," 514.

545 Medical Correspondent, "An X-Ray Crisis: The Danger to Reproduction, New Epoch in Radiography," *The Times (London)*, March 31 1921, 10.

546 "The Danger of X-Ray Therapeutics," 9.

547 Ibid.

technical and biological.[548] The medical correspondent writing for *The Times* echoed this sentiment:

> The lowering of resistance which may follow such prolonged exposure must be studied and provided against. Perhaps most important of all is further intensive investigation of the physical properties of the rays themselves. Enough is known to suggest that they are complex; ways of disentangling them and examining their different actions must be elaborated.[549]

Sounding the alarm, this writer went on to declare: “It does not seem to be understood as yet that the sad death of Dr. Ironside Bruce constitutes … a crisis in x-ray medicine the possibility of which has scarcely been dreamt of.” Noting that it was well known that “most x-ray workers of the early days are childless,” this author argued that the greatest danger was due to potential damage to the hereditary material.[550] (S)he went on to explain the danger using an incorrect physics model, explaining that both x-rays and radium emitted three kinds of waves, alpha, beta and gamma, telling readers that it was the gamma rays, or hard rays that caused the death of reproductive cells and were the hardest to protect against.[551]

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548 "Research in Radiology," *British Medical Journal* 2(1921): 500.

549 "The Danger of X-Ray Therapeutics," 9.

550 This was later confirmed by Muller’s famous experiments on the mutating effects of x-rays on fruit flies in 1926.

551 "The Danger of X-Ray Therapeutics," 9.

Table 3: British X-Ray and Radium Protection Committee 1921

Sir H. Rolleston (Chairman)	Doctor	Past President of the Royal College of Physicians
Dr. Stanley Melville (Secretary)	Doctor	Royal Society of Medicine
Professor Sidney Russ (Secretary)	Physicist	Institute of Physics
Sir Archibald Reid	Doctor	British Institute of Radiology
Dr. Robert Knox	Doctor	British Institute of Radiology
Dr. G. Harrison Orton	Doctor	Röntgen Society
Dr. J. C. Mottram	Doctor (hematologist)	Radium Institute
Dr. G.W.C. Kaye	Physicist	National Physical Laboratory
Dr. S. Gilbert Scott	Doctor	Royal Society of Medicine
Mr. Cuthbert Andrews	Manufacturer	Röntgen Society

Amidst this growing wave of worry, the physician Robert Knox wrote a letter to *The Times* announcing the formation of the British X-Ray and Radium Protection Committee to look into questions of safety. The committee consisted of volunteers from the primary medical and radiological societies. It did not receive official funding from any one medical or radiological organisation and at times, the members supplied their own money to fund

their work, although they were primarily sustained by grants and donations.[552] The members of the committee are shown in Table 3. [553]

The committee was composed of 10 members: 7 doctors, 2 physicists and a representative of the manufacturing community. Cuthbert Andrews later remembered a division of labour on the committee with “the medical aspects of the problem,” being “of course, the subject of discussion mainly between the radiologist members, while the physics side was to all intents and purposes in the hands of Kaye and Russ.”[554] But overall, despite being vastly outnumbered, Andrews recalled that “generally the physicists were the most vocal of the members.” He described Knox, Harrison, Orton and Scott as “reserved” and Mottram, the hematologist as “worried.”[555]

The first set of recommendations came out only 3 months after this committee was formed, published in July 1921 in the *Journal of the Röntgen Society*.[556] These recommendations were subsequently revised and expanded twice more in the next seven years. Although hospitals were strongly urged to follow the guidelines, the recommendations remained voluntary and did not prompt official legislation in England on radiation safety. The Ministry of Health and the Ministry of Pensions did, however, express

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552 Cuthbert Andrews, "The First Years," *The British Journal of Radiology* 26(1953): 556.

553 Spear, "The British X-Ray and Radium Protection Committee," 553.

554 Andrews, "The First Years," 556.

555 Ibid.

556 "X-Ray and Radium Protection," *Journal of the Röntgen Society* 17(1921).

support for the recommendations, and x-ray dermatitis was included in the Workman's Compensation Act in 1924.[557]

The recommendations made by the Committee were intended to address the safety of the x-ray operator and were not concerned with the health of the patient. It was agreed that the power of the x-rays had "outstripped the protective measures in the majority of installations in this country,"[558] but "the existing measures had proved to be adequate so far as the patient was concerned."[559]

The committee faced numerous challenges in coming up with any set of comprehensive guidelines. In 1921, there was no agreed upon unit of measurement for the quantity of x-rays, no consensus as to the cumulative effects of radiation on the body, or whether the wavelength of the x-rays affected their biological action, and there was continued uncertainty as to the extent to which particular individuals exhibited idiosyncratic responses to x-rays or developed sensitivities after prolonged exposure.[560] Looking back on this uncertainty, Kaye later explained that these guidelines were based primarily on physics research into the penetrability of various materials and estimates of the exposures received by x-ray workers who had remained healthy. He recalled,

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557 G.W.C Kaye, "Protection and Working Conditions in X-Ray Departments," *British Journal of Radiology* 1(1928): 295.

558 ———, "Radiology and Physics," 41.

559 Ibid., 41.

560 ———, "Protection and Working Conditions in X-Ray Departments," 295..

> the protective values recommended had necessarily to be framed, mainly from the point of view of physical measurements, and the accumulated experiences of a considerable number of older workers. It was obvious that the protective values selected would have to be in the nature of a compromise, as considerations of weight and cost would preclude any attempt at stopping the rays virtually completely. Some small amount of radiation is bound to reach the operator, particularly during screening, but this should be made so small as to be innocuous.[561]

Sidney Russ had a similar recollection, explaining that these first recommendations were "rather general" due to the lack of an x-ray unit.[562]

None of this uncertainty was evident in the first set of guidelines published in July of 1921. The first statement of these guidelines proclaimed with complete confidence that, "The danger of over-exposure to X-rays and radium can be avoided by the provision of efficient protection and suitable working conditions."[563] X-ray and radium workers were not to work longer than 7 hours per day, with Sundays and two half-days off per week in addition to one month of vacation per year. Workers were to make use of available protective equipment and the committee gave specific, quantitative recommendations about the equivalent thickness of lead that was satisfactory for different kinds of work. For diagnostic purposes and superficial therapy, the x-ray bulbs should be surrounded by at

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561 Ibid., 297.

562 Russ, "A Personal Retrospect," 554.

563 Reprinted in Kaye, *X-Rays*: Appendix V.

least 2 mm of lead, for deep therapy this should be increased to 3 mm of lead. Doctors performing screen observations should use gloves that provided protection equivalent to at least ½ mm of lead and protective screens equivalent to at least 2 mm of lead whenever possible. The committee also gave guidelines for proper ventilation and electrical precautions. The only admission of uncertainty came at the end of the document. Here, the committee recommended periodic blood tests of all workers "in view of the varying susceptibilities of workers to the radiation."[564]

Reaction in the medical community to these guidelines was predictably mixed. Some felt that the measures were "inadequate," while others complained of " heavy, unwieldy and cumbersome apparatus."[565] Kaye acknowledged the inconvenience caused by these guidelines, the potential loss of suppleness for instance due to increased lead in gloves, and the extra expense created for hospitals needing to buy new or additional protective devices. He tried to motivate hospital administrators and doctors with the specter of insurance companies who were increasingly viewing x-ray workers as risky clients. Because of the growing hesitation of these companies to insure radiologists, hospitals "should err on the side of safety."[566]

For potential patients, the existence of these guidelines was comforting and by 1925, the tone in the press had changed considerably. The frightened journalists proclaiming a crisis in radiology only four years previously had given way to new voices

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[564] Ibid.

[565] ———, *Roentgenology: Its Early History, Some Basic Principles and the Protective Measures*: 78.

[566] Ibid., 111.

making calm pronouncements that complete safety had been achieved. In a piece reporting on the International Congress of Radiology in 1925, a journalist quoted Mr. C. Thurston Holland, who had proudly stated that, "It was now possible for a young man to start on a career of radiology without suffering any risks at all."[567] The reporter went on to say that "an examination of the excellent display of up-to-date apparatus … shows that precautionary measures have now been carried to perfection."[568] Manufacturers, only too happy to add to this renewed sense of confidence, echoed these sentiments. In a 1926 pamphlet aimed at the public, the Victor X-Ray Corporation assured readers that,

> modern equipment and modern methods have every provision for the protection of both operators and patients against the harmful or secondary rays, so that in the present day x-ray burns can occur only through gross negligence or ignorance. Consequently, when we submit ourselves for x-ray examination to a competent roentgenologist, we need feel no fear of harm from the comparatively small amount of primary rays required in the production of a radiograph, or in the usual fluoroscopic exam.[569]

The panic from earlier in the decade was forgotten.

When the Second International Congress of Radiology met in 1928 in Stockholm, a set of International Recommendations for X-Ray and Radium Protection were adopted

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[567] "Radiologists Meet," *Evening Post*, August 11 1925, 9.

[568] Ibid.

[569] Victor X-Ray Corporation, *A Little Journey into the Realms of the X-Ray*: 23-24.

based on the British guidelines. By this time, many countries had x-ray safety committees (Table 4), and there was a sense that international cooperation was desirable in order to come up with a unified set of rules. The British X-Ray and Radium Protection Committee offered their guidelines as a potential template and sent George Kaye, the National Physical Laboratory physicist, as their spokesperson, demonstrating again his leadership in this area and the perceived expertise of physicists in questions of safety. The British Guidelines were adopted as the basis for the International Guidelines, and these in turn informed the first national recommendations in the United States, discussed below.[570]

Table 4: National Safety Committees c.1928[571]

England	X-Ray and Radium Protection Committee
United States	The Safety Committee of the American Roentgen Ray Society
Germany	The German X-Ray Society
Sweden	The X-Ray and Radium Protection Committee
Russia	The Radiological Congress of the Soviet Federation
Holland	The Protection Committee of the Board of Health

570 For easy comparison, both the 1927 (3rd revision) British Guidelines and the first 1928 International Guidelines are reproduced in Appendix A and B of Kaye, "Protection and Working Conditions in X-Ray Departments."

571 Ibid., 295-96.

Prior to the 2nd International Congress, the United States did not have a safety committee that was nationally representative. The American Roentgen Ray Society had a safety committee, but the Radiological Society of North America did not. National coordinated action on safety was organized by Lauriston Sale Taylor, a young physicist who had been hired by the National Bureau of Standards in 1927. In the United States, the National Bureau of Standards took on the same central role as the National Physical Laboratory in Britain. Although, like the National Physical Laboratory, the Bureau was not at this time an official regulatory agency of the federal government.[572] Lauriston Taylor had recently graduated with an undergraduate degree in physics from Cornell and was working on his PhD requirements when he accepted this position. He was new to the world of medical x-rays, having been, in his recollection, "booby-trapped into it."[573] He was hired by the Bureau to look into issues of x-ray measurement and did not receive much advice about protecting himself. He remembered being told not stand in front of the beam, but little else. In order to try to estimate the radiation reaching him, he made a belt of photographic film to wrap around the tube that he was using for research. Every piece of film turned black and he even measured radiation over at his desk, which sat quite a distance away from the tube. This was all the more disturbing given that the tube was lined with a protective lead shield and then covered with a big brass cylinder. Eventually Taylor

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572 The Bureau did not get federal governmental controls until the 1950s. See Taylor, "X-Ray Measurements and Protection, 1913-1964," 18.

573 Interview of Lauriston Sale Taylor by Gilbert Whittemore on August 11, 1990, Niels Bohr Library & Archives, American Institute of Physics, College Park MD, USA. http://www.aip.org/history/ohilist/5153_1.html.

realized that the lead lining had been fastened to the outer brass cylinder with little brass screws and that x-rays were leaking out wherever there was a screw.[574]

Table 5: US Advisory Committee on X-Ray and Radium Protection 1929

Lauriston Taylor	Physicist	International Safety Commission; National Bureau of Standards
H.K. Pancoast	Doctor	American Roentgen Ray Society; University of Pennsylvania Hospital
J.L. Weatherwax	Physicist	American Roentgen Ray Society; Philadelphia General Hospital
R.R. Newell	Doctor	Radiological Society of North America; Stanford University Hospital
G. Failla	Physicist	Radiological Society of North America; Memorial Hospital
Francis Carter Wood	Doctor	American Medical Association; St. Luke's Hospital, New York
W.D. Coolidge	Physicist	Manufacturing; General Electric
W.S. Werner	Business	Manufacturing; Kelley-Koett Man. Co.

At the Bureau, he met Kaye who was visiting the United States and Kaye invited him to attend the International Congress of Radiology in Stockholm. Taylor was more than happy to accept and came back with recommendations from Kaye to start working on a unified set of American safety standards. Taylor contacted the presidents of the two radiological societies, the American Roentgen Ray Society and the Radiological Society of

574 Ibid.

North America, asking each of these bodies to nominate one physicist and one radiologist to sit on a new, national safety committee. Two members were also nominated from the manufacturing community and one from the American Medical Association. Taylor was the final member, representing both the International Safety Commission and the National Bureau of Standards on the committee.

The resulting Advisory Committee on X-Ray and Radium Protection was more evenly split than the British X-Ray and Radium Protection Committee, containing four physicists, three doctors and one businessman (see Table 5).[575] The committee published an initial set of recommendations in 1931. These appeared in the American radiological journals and in an official National Bureau of Standards Handbook.[576] This first handbook was updated in 1936 and was joined by a Handbook on Radium Safety in 1934 and a Handbook for the Safe Handling of Radioactive Luminous Compounds in 1941.[577]

Table 6 shows the minimum equivalent lead thicknesses that were deemed by the American Committee in 1931 to be “adequate” protection for each corresponding exciting voltage. These numbers were identical to those set out by the International Committee, with the only difference being that the American table continued up into higher voltages,

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575 "X-Ray Protection," ed. U.S. Department of Commerce: Bureau of Standards (Washington: United States Government Printing Office, 1931).

576 Ibid. reprinted in *Radiology* 17 (1931) p.542 and *The American Journal of Roentgenology and Radium Therapy* 26 (1933) p.436.

577 Ibid.; "X-Ray Protection," ed. U S Department of Commerce: National Bureau of Standards (Washington DC: United States Government Printing Office, 1936).; "Radium Protection for amounts up to 300 milligrams," ed. U.S. Department of Commerce: National Bureau of Standards (Washington DC: United States Government Printing Office, 1934).; "Radium Protection," ed. U.S. Department of Commerce: National Bureau of Standards (Washington DC: United States Government Printing Office, 1938).; "Safe Handling of Radioactive Luminous Compound," ed. U.S. Department of Commerce: National Bureau of Standards (Washington D.C.: United States Government Printing Office, 1941).

Table 6: **Lead Thicknesses for protective shields recommended in 1931** [578]

X-Rays generated by peak voltages not in excess of – (KV)	Minimum equivalent thickness of lead (mm)
75	1.0
100	1.5
125	2.0
150	2.5
175	3.0
200	4.0
225	5.0
300	9.0
400	15.0
500	22.0
600	34.0

giving a minimum equivalent lead thickness of 34 mm for 600 KV. The recommendations in the 1936 NSB handbook were the same. Along with these tables, the handbook gave rules for electrical safety as well as rules for the safe handling of inflammable anaesthetics like ether, ethylene and propylene that could explode in the presence of an electric spark. As in Britain, there was no official legislation enforcing these guidelines. The National Bureau of

578 Ibid., 3.

Standards nonetheless strongly advised that, “A copy of these rules shall be given to every x-ray worker whether temporary or otherwise upon entering service. A signed receipt that the rules are understood shall be given to the physicians in charge of the department.” And with electrocution continuing to represent the greatest immediate danger, a copy of the first aid procedures for electric shock was to be posted in all x-ray rooms.[579]

There was, however, still a sense in these regulations that the rules were provisional and that there was much that was unknown. While doctors and x-ray workers were instructed that “The omission of any protective device for the sake of expediency of operation is strictly forbidden,” they were also warned that,

> Protective gloves and aprons, even though in compliance with these recommendations, do not afford adequate protection. Consequently, care shall be taken at all times not to expose the body unnecessarily to radiation.[580]

Radioscopic work should be performed in the shortest possible time and with the lowest x-ray intensity possible. All x-ray workers were to test lead rubber for cracks because of the tendency of rubber to become brittle with age. The “Rules for the Physician in Charge” included an admonition that the efficacy of x-ray protective devices should be checked yearly with a fluorescent screen closely fitted over the eyes. “The detection of any fluorescence by the well, dark adapted eye shall be considered a hazard and remedied.” But these precautions were still not sufficient. The doctor in charge of the x-ray department

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[579] "X-Ray Protection," 22-26.

[580] Ibid., 23.

was to ensure that at least every 4 months every worker would be supplied with dental x-ray film to be worn for 15 working days. Any darkening of this film would be evidence that the protective measures were not adequate. But even that extra measure was not enough. Every worker must also be given 4 weeks vacation and have complete blood counts done every two months with the results kept on permanent record in order to track any unusual changes.[581]

Evident in this document is the tension between physics and medicine, between a completely standardized, quantified and quantifiable problem and a body of idiosyncrasy and uncertainty. The lead gloves and lead aprons stamped with a particular lead equivalency provided comfort but the blood tests and the dental film tests all spoke to doubt about the reliability of equipment, the efficacy of the rules and the possibility of personal susceptibility.

When the safety guidelines were published in handbooks, textbooks and journal articles, there was no justification given for the particular numbers shown in Table 4, but they were often published in conjunction with physics research in a way that suggested that their rationalization lay within the physics lab. The 2 mm of lead shielding deemed safe for voltages up to 125kV had been a standard guideline for diagnostic work since the original 1915 British regulations. To scale this number up for higher voltages and to determine the equivalent thicknesses of different kinds of protective materials required research into the absorption and transmission of x-rays. This kind of work was done by Kaye at the National Physical Laboratory and Taylor at the National Bureau of Standards.

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[581] Ibid., 26.

Figure 43 shows a summary of Kaye's research, published in *The British Journal of Radiology* in 1928 alongside the internationally agreed upon safety recommendations.[582] The first table shows the reduction in the intensity of x-rays transmitted through different thicknesses of lead at different exciting voltages (measured using the ionisation method described in Chapter 4). Kaye had measured, for instance, that 2.0 mm of lead would transmit 0.0103% of x-rays produced at 100KV. If we correlate these measurements with the safety guidelines in Table 4, it is possible to find the percentage of x-ray energy transmitted for some of the recommended thicknesses of lead. The guidelines stated, for instance, that an x-ray bulb excited to 200 KV required 4 mm of lead as protection. Kaye's research showed that at this voltage, 4 mm of lead transmitted 0.00196% of the incoming x-rays. But in order for these numbers to begin to be physiologically meaningful, Kaye would have had to translate these percentages into absolute intensities in röntgens. He didn't do this. In this paper, and in his book, *X-Rays*, he presented the safety guidelines along side this transmission data but left the actual link between the two unexplained.[583] In the paper, he merely told his readers that "a reduction of intensity to 1/10000 occurs with a 2mm. lead screen at 100 KV., and with a 3mm. lead screen at 200 KV."[584] Certainly that would have sounded reassuring to the doctors who were being asked by Kaye to trust that this kind of reduction all but eliminated the received dose of x-rays. However, Kaye wasn't able to lay out a set of clear steps leading from his research to the published guidelines, because these steps simply didn't exist.

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[582] Kaye, "Protection and Working Conditions in X-Ray Departments," 295-312.

[583] ———, *X-Rays*: 175.

[584] ———, "Protection and Working Conditions in X-Ray Departments," 302.

TABLE I

TRANSMISSION OF X-RAYS BY LEAD

Thickness of lead. mm.	70 KV. X-rays first filtered by 0·2 mm. celluloid.	70 KV. X-rays first filtered by 1·2 mm. aluminium.	100 KV. X-rays first filtered by 0·2 mm. celluloid.	100 KV. X-rays first filtered by 1·2 mm. aluminium.	200 KV. X-rays first filtered by 0·2 mm. celluloid.	200 KV. X-rays first filtered by 1·2 mm. aluminium.
0	1	1	1	1	1	1
0·2	0·0182	0·0407	0·0502	0·154	0·125	0·180
0·4	0·00325	0·00748	0·0175	0·0573	0·0533	0·0748
0·6	0·000805	0·00213	0·00741	0·0252	0·0264	0·0361
0·8	0·000257	0·000741	0·00343	0·0123	0·0141	0·0194
1·0	0·0000944	0·000294	0·00170	0·00612	0·00791	0·0109
1·2	0·0000391	0·000126	0·000891	0·00321	0·00470	0·00632
1·4	0·0000177	0·0000573	0·000491	0·00177	0·00286	0·00373
1·6	0·00000851	0·0000271	0·000282	0·00103	0·00179	0·00225
1·8	0·00000430	0·0000136	0·000169	0·000617	0·00114	0·00140
2·0	0·00000227	0·00000708	0·000103	0·000380	0·000736	0·000883
2·5	—	—	0·0000336	0·000124	0·000255	0·000294
3·0	—	—	0·0000119	0·0000445	0·0000957	0·000111
3·5	—	—	—	—	0·0000401	0·0000475
4·0	—	—	—	—	0·0000196	0·0000234
4·5	—	—	—	—	0·0000108	0·0000128
5·0	—	—	—	—	0·00000631	0·00000748
5·5	—	—	—	—	0·00000394	0·00000462

TABLE II

X-RAY PROTECTIVE MATERIALS

Material.	Mean Density.	Lead Equivalent.	Equivalent thickness of material in mm. 50 KV.	100 KV.	150 KV.	200 KV.
	gm./cc.	mm.	mm.	mm.	mm.	mm.
Aluminium	2·7	1	96	60	65	70
		2	—	120	130	140
		3	—	180	195	210
Brass	8·4	1	6·5	4·5	6·0	6·5
		2	—	9	13·5	16
		3	—	14	21·5	27
		4	—	19	30	40
Steel	7·8	1	11·5	6·5	9·5	11·5
		2	—	15	21·5	25
		3	—	23·5	34	39
		4	—	32	47	53

Figure 43: Table I shows the transmission of x-rays through different thicknesses of lead at different exciting voltages. Table 2 shows the thicknesses of material required to achieve particular levels of protection for different voltages.[585]

[585] Ibid., 301 and 05.

We have already seen that in Kaye's recollection, the guidelines were based on a best guess:

> The best the committee could do was try and translate into specific recommendation a sort of grand average of the protective measures which could be gleaned from the working conditions of a number of experienced radiologists who had escaped injury and still enjoyed normal health.[586]

Kaye's work was of course helpful in determining the lead equivalency of different materials, shown in the second table in Figure 31. But the practical difference in the health of an operator who stood behind 2mm versus 3 mm of lead was unknown. Whether knowingly or not, by presenting the guidelines alongside his own research, Kaye ensured that the doctors uninvolved in the setting of these safety standards didn't see the guess. These doctors saw a reassuring table of protective values backed up by Kaye's precise physical measurements.

Questions about whether this level of protection was actually adequate had to be answered after the fact. In 1928, 7 years after the initial guidelines were published by the British X-Ray and Radium Protection Committee, Kaye was quite happy to note that "such quantitative biological evidence as has been advanced since the recommendations were drafted, is not unfavourable to the committee's suggested protective values."[587] Surveying the literature, he found that there was a range of estimates of what constituted a "safe" amount of radiation. Surveying healthy radiologists and estimating their exposure levels,

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[586] ———, "The Story of Protection," *Radiography* 6(1940): 299.

[587] ———, "Protection and Working Conditions in X-Ray Departments," 299.

the American researcher A. Mutscheller argued that workers should not be exposed to more than 1/1000th of an erythema dose in 3 days.[588] The Dutch Board of Health was more cautious, and recommended that this level not be exceeded over the course of 15 days, and the French doctor, Solomon, was more generous, allowing this same exposure over only 0.3 days.[589] Kaye came up with an average safe dose based on these estimates and demonstrated that the British Guidelines more than adequately met this benchmark.

Following the adoption of the röntgen in 1928, the feeling, according to Sidney Russ was that "the röntgen could be made to speak for itself." But he went on to note that, "It was rather a shot in the dark because there was at that time, I believe, no biological entity whose reaction to a dose of 1r was known …"[590] At the 4th Annual Congress of Radiology in Zurich in 1934, the safety recommendations were streamlined and radiologists were told that no matter what voltage they were working with:

> The evidence at present available appears to suggest that under satisfactory working conditions a person in normal health can tolerate exposure to x-rays to an extent of about 0.2 international röntgens per day.[591]

Of course these standards were slow to trickle into practice. A text book by the Ritter Dental Company published in 1933 still used the older terminology, noting the

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[588] *Amer. Journ. Roent.* 13, 16, 1925

[589] Kaye, "Protection and Working Conditions in X-Ray Departments," 300.

[590] Russ, "A Personal Retrospect," 555.

[591] "International Recommendations for X-Ray and Radium Protection," *British Journal of Radiology* 7(1934): 695.

recommendation that an operator “never exceed one-half the erythema dose or 600 milliampere seconds,” and reassured dentists that “the exposures we have recommended are well within this limit.”[592]

5.4 Safety Inspectors

The perception that the internationally adopted guidelines for the safe use of x-rays were based in large part on physics research helped to reinforce physicists’ expertise, but when actual physicists showed up in hospitals and medical clinics to inspect the safety devices, their authority was all the more immediate.

In the United States, we have seen that work to inspect the efficacy of various safety devices was undertaken by the National Bureau of Standards as early at 1918. Gorton, one of the physicists working at the NBS, collaborated with several manufacturers to increase the quality of materials and found that these companies were able to increase the protective value of lead glass by 80% and lead rubber by 100%.[593] And as the Bureau took on more x-ray research, it was actively approached by manufacturers to test equipment. But this kind of work did not become the primary mandate of the agency. Lauriston Taylor remembered that, in the first place, “one couldn’t really see a need for it and, two, it looked like that was getting us into an industrial testing and certification program, which was not

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[592] Ritter Manufacturing Company, *A Text Book of X-Ray Technique* (Rochester, NY: Ritter Dental Manufacturing Company, 1933), 60.

[593] E.B. Rosa, "Lead Glass and Lead Rubber - U.S. Bureau of Standards," *American Journal of Roentgenology* 5(1918): 188.

the sort of thing the Bureau engaged in then."[594] The 1931 NSB handbook on x-ray safety recommended that an "annual inspection of x-ray protection be conducted by an expert in such work," but did not specify who that expert should be. The same handbook also recommended that "All x-ray protective material shall be indelibly marked by the manufacturer in such a manner as to readily show the lead equivalent thickness of the material,"[595] putting the onus on the manufacturers to ensure that their materials met the NSB standards.

In Britain, the National Physical Laboratory (NPL) did actively take on this role. The NPL was founded in 1900, and was initially overseen by the Royal Society, but became part of the Department of Scientific and Industrial Research in 1918. Its self-described mandate was, "the accurate determination of physical constants, to establish and maintain precise standards of measurement, and to make tests of instruments and materials."[596] The Physics Department, led by Kaye, worked on a number of medically relevant projects, including the testing of clinical thermometers. This department had charge of the British Radium Standard and did work on measuring x-ray intensity and dosage, as well as x-ray crystallography. The British X-Ray and Radium Protection Committee collaborated with the NPL, asking them to undertake physical research and inspect hundreds of hospitals.[597] The

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594 Interview of Lauriston Sale Taylor by Gilbert Whittemore on August 11, 1990, Niels Bohr Library & Archives, American Institute of Physics, College Park MD, USA. http://www.aip.org/history/ohilist/5153_1.html.

595 "X-Ray Protection," 3.

596 *The National Physical Laboratory: A Short Account of its Work and Organization*, (1939), 3.

597 Kaye, "Protection and Working Conditions in X-Ray Departments," 297. Also mentioned in Spear, "The British X-Ray and Radium Protection Committee," 553.

NPL agreed and continued this inspecting role well into the late 1930s.[598] In the published guidelines for doctors, this role became more definite over time. In the first Memorandum published by the British X-Ray and Radium Protection Committee in July 1921, the Committee merely pointed out, "that the National Physical Laboratory, Teddinton, is prepared to carry out exact measurements upon X-Ray protective materials, and to arrange for periodic inspection of existing installations."[599] Only 5 months later the tone was much firmer:

> In view of the widespread uncertainty and anxiety as to the efficacy of the various devices and materials employed for the purposes of protection … the X-ray and Radium Protection Committee strongly advises that … the various protective appliances should be inspected and reported on by the National Physical Laboratory."[600]

The Laboratory would engrave the NPL monogram on a piece of equipment to certify that it met the requirements of the Committee. Because of material deterioration, this inspection was offered every 12 months. The recommendations, which were given in terms of the "lead equivalent thickness" required this kind of intervention. Doctors were not equipped in any way to test the lead equivalency of their materials. The Committee also recommended that the NPL be involved in the early planning of an x-ray department to ensure a safe layout and proper ventilation. And they hoped that manufacturers would

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598 *The National Physical Laboratory: A Short Account of its Work and Organization*: 4-6.

599 Kaye, *X-Rays*: Appendix V.

600 Ibid., Appendix V.

likewise comply by seeking out the NPL certificate verifying that adequate safety standards had been met.[601]

Of course this testing had to be paid for by the hospital.[602] Kaye later remembered some critics found the recommendations "unnecessarily drastic" "heavy, clumsy and costly, and too cramping."[603] Noting this problem, Kaye hoped that even though, "the Hospitals are chary of spending money just now … as the big London hospitals are giving a lead we may expect many others to seek similar assurance on the score of X-Ray protection."[604] Kaye pointed out "how often our reports have been instrumental in influencing a situation," for instance in helping a radiologist persuade a resistant board of governors to pay for new equipment or increased protection. At first he explained this persuasive power by appealing to the official status of the NPL as a government body, noting "that the impartial position of the Laboratory as a Government institution has proved of assistance in this inspecting work."[605] In a later revision of this argument, Kaye additionally pointed to "the standing of the laboratory,"[606] perhaps invoking directly the cultural capital of physicists.

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[601] Ibid., 295.

[602] In *The National Physical Laboratory: A Short Account of its Work and Organization*: 3. The author explains the fee structure for industrial consulting and testing work.

[603] ———, "The Story of Protection," 300.

[604] ———, "Radiology and Physics," 42.

[605] ———, "Protection and Working Conditions in X-Ray Departments," 297.

[606] Ibid., 75.

Kaye gave a speech to the British radiological community in 1922, characterising the state of most radiological departments as worryingly unsafe, justifying the NPL inspections, and motivating other departments to take advantage of the NPL program. Kaye was blunt:

> A small proportion of the departments were reasonably satisfactory, the majority were very unsatisfactory. In some instances the protection for the operator was lamentable in its inadequacy. We have taken with us in our inspection work delicate electroscopes, with the object of exploring the extent of the scattered radiation in different parts of the X-ray rooms. In some cases it was impossible to use them at all; we had to content ourselves with noting the comparative ease with which we could see the bones of the hand on a screen as it was carried around the room – and this with the protective appliances in full action.[607]

Given that "such measurements are rapidly and inexpensively carried out by the Laboratory," Kaye hoped that the audience agreed that, "no radiologist need deny himself the security which the N.P.L. certificate affords."[608]

In this same speech, Kaye had nothing but praise for the manufacturing community for cooperating.

> Almost all of [the manufacturers] have taken steps to obtain from the National Physical Laboratory test figures for the various protective materials which they are incorporating into existing and new installations.[609]

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607 ———, "Radiology and Physics," 42.

608 Ibid., 43.

To motivate manufacturers and radiologists even further, he reported on the materials research done by the NPL that had uncovered a huge variation in samples of lead glass. So big, in fact, that, "a manufacturer can easily be 100 per cent out in his reckoning if he employs uncertified material. He owes it to himself and his customers to take no such risk."[610]

This role of the NPL as arbiter of radiological safety was firmly in place by the mid 1920s. This is evident in Kaye's reports on the hundreds of departments he inspected, but it came up as well in an even more public setting, in concerns raised over commercial machines used to judge the fit of shoes. Pedoscopes were a modified fluoroscope which would allow a salesperson in a department store to see where the bones of the foot were in relation to the end of a shoe. They were extremely popular in the decades before WWII, used from 1920s, and only began to be banned in the 1950s.[611]

In late December of 1925, Christina Jordan, the wife of British radiologist Alfred Jordan, wrote in to *The Times* to warn the public about the new shoe-fitting x-ray machines. Pointing to a notice on the side of the machine that read, "Not to be used for more than 30 seconds," she argued that "this in itself is an admission that the use of the machine is fraught with danger." She worried that the assistants in the shops running the machines

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[609] Ibid.

[610] Ibid.

[611] Jacalyn Duffin and Charles Hayter, "Baring the Sole: The Rise and Fall of the Shoe-Fitting Fluoroscope," *Isis* 91(2000): 260-82.

were putting themselves in danger, noting that "everything upon which the x-rays fall gives off secondary rays, and no system gives complete protection against these secondary rays." "We admire a 'martyr to science' but a 'martyr to commerce' stands on a very different footing."[612]

This provoked a series of letters to the editor in the following weeks. The first from Charles Baber, who said that he and his assistants had used the machine since 1921 with no ill effect. He argued that the risk from ill-fitting shoes was much greater than the risk from x-rays.[613] Joining the defense of the shoe x-rays was J. Edward Seager, Secretary of X-Rays Limited, the company who manufactured the Pedoscope in Britain. He protested that the 30 second warning "is attached to our pedoscope solely for the purpose of avoiding any risk or damage to the apparatus by over running the x-ray tube, and has nothing whatever to do with the question of any risk to the user." He went on to say,

> The fact that the pedoscope has been tested by the National Physical Laboratory and certified by them as fully complying with all the requirements of the X-ray and Radium Protection Committee whose certificates we hold, should be conclusive evidence that there is no danger whatever to either assistants or users of the pedoscope from direct or secondary radiation.

But just in case the authority of the physicists at the NPL wasn't enough, he pointed out that the "practical proof" of its "absolute safety" lay in the fact that it had been used for five

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[612] Christina Jordan, "Shoe-fitting X-Rays," *The Times (London)*, Dec. 29 1925, 11.

[613] Charles H. Baber, "Shoe-fitting by X-rays," *The Times (London)*, Dec. 30 1925, 6.

years throughout the British Empire without (he claimed) a single customer complaint or damage to a shop assistant.[614]

The final person to write in was a surgeon, who wrote in anonymously, to give his medical opinion that the pedoscope couldn't possibly be useful. Further, he noted, that may x-ray workers believed "there [were] still unknown dangers." He took the debate again to the National Physical Laboratory, citing a pamphlet issued by the NPL in 1924 which referred to anxiety over the efficacy of safety devices. He pointed out the necessity of having materials like rubber inspected yearly. But in the end he moved back to his expertise as a doctor, noting the long periods of time that might elapse before damage to the worker became visible. "Personal idiosyncrasy and the number of exposures play a very important role," he argued, and the effects were cumulative, even with long breaks between exposures. [615]

In this exchange, the National Physical Laboratory was invoked both in defense of the pedoscope and to raise the specter of deteriorating materials. Its presence in the debate at all illustrates the central role played by the British physicists at the NPL in matters of x-ray safety.

614 J. Edward Seager, "Shoe-fitting by X-Rays," *The Times (London)*, Dec. 31 1925, 6.

615 FRCSI, "Shoe-fitting by X-rays," *The Times (London)*, Jan. 04 1926, 8.

5.5 Reassurance and Resistance

Mr. Law, a senior radiographer at the Edinburgh Royal Infirmary started working in the x-ray department in 1900 at the age of 16. Sadly he was never in good health. When WWI broke out, his work at the hospital was interrupted, and despite the stress of military life, he actually gained weight and felt much healthier. He lost the weight again when he returned to his hospital job but then gained it back again once the hospital started following the new British guidelines for the safe use of x-rays in the early 1920s. Kaye, who told this story as anecdotal evidence in support of the efficacy of those guidelines, attributed Law's improved health to better protection, improved ventilation and reasonable working hours and holidays.[616]

When x-rays were first discovered, doctors and physicists were united in ridiculing fears about their impropriety and ghostliness, voicing instead growing concern about the burns and epilation caused by the rays. When the widely available lead shielding and rules for maximum exposure times appeared to solve the problem of unwanted burns, the expertise of physicists was neither offered nor sought. But as physicists began to sound the alarm about lead glass that offered no greater protection than normal glass, and lead rubber that grew brittle and ineffective with age, radiologists began to worry again about their health. When they began to die of anemia, the problem became urgent. Without any consensus as to what constituted a safe dose of x-rays, the first guidelines about the appropriate thickness of lead had to be a best guess. But these first guesses looked definitive. Doctors were told, for instance, that 2mm of lead was sufficient protection for

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616 Kaye, *Roentgenology: Its Early History, Some Basic Principles and the Protective Measures*: 108-09.

diagnostic work. The later International guidelines were even more exact, giving a table of equivalent thicknesses of lead sufficient to provide protection at multiple different voltages. These charts portrayed an even greater surety, even if routine blood tests for x-ray workers were still strongly suggested. This reassuring table of numbers appeared to come directly from physics research, and so deference to the expertise of physicists was embedded in the adopted safety guidelines. This deference was strengthened when actual physicists were called on to police the new standards. The physicists who checked the efficacy of safety arrangements in hospitals possessed the necessary testing equipment and the expert knowledge to justify the safety guidelines that stood between doctors and unwanted burns, anemia and death.

But each wave of optimism was followed by a wave of concern. The deaths of radiologists and the very public deaths of the radium dial painters beginning in the mid 1920s had increased public concern about radiation safety.[617] But with international recommendations in place to prevent these tragedies, Kaye could write in 1928 that "Radiology is, in fact, no more dangerous, under proper conditions, than scores of other professions."[618] In 1936, Percy Brown wrote his book commemorating his colleagues who were horribly injured and who had died because of exposure to x-rays. Yet despite the gruesome subject of his book, Brown still felt confident declaring "The operator and the patient now share total immunity from [x-rays'] adverse effects."[619]

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[617] Ross Mullner, *Deadly Glow: The Radium Dial Worker Tragedy* (Washington D.C: American Public Health Association, 1999).

[618] Kaye, *Roentgenology: Its Early History, Some Basic Principles and the Protective Measures*: 113.

[619] Brown, *American Martyrs to Science Through the Roentgen Ray*: 265..

In a post-Hiroshima world, the strands of resistance to the idea of any safe amount of radiation became even stronger. In 1953, hospital physicist Sidney Russ argued that it was not safe for everyone to work with radiation. “Every now and again one in a group of people is found unsuitable for the work; the person has an idiosyncrasy towards ionizing radiation and systematic blood counts alone can detect it.”[620] But it was Alice Stewart, a British epidemiologist, who shattered the perception that diagnostic x-rays could ever be perfectly safe. In the 1950s she showed that a single diagnostic x-ray performed on pregnant women, a procedure delivering a dose much smaller than the accepted minimum safe dose of x-rays, was enough to significantly increase the rates of pediatric leukemia in the children delivered by those mothers. Yet Stewart came up against huge opposition when she announced these findings. [621] The certainty of a minimum safe dose—the certainty provided by physicists’ sharp numbers—has been, and continues to be, hard to abandon.

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[620] Russ, "A Personal Retrospect," 555..

[621] Gayle Greene, *The Woman Who Knew Too Much: Alice Stewart and the Secrets of Radiation* (Ann Arbor: The University of Michigan Press, 1999), 78-93. Critics of Stewart cited data from the studies following the atomic bomb survivors. That data had shown no such link between maternal x-rays and childhood cancers. But Stewart argued that this atomic bomb data was skewed because it only included children who had been exposed in utero who had survived. The children with leukemia surely would not have survived long enough to be included in the study.

Epilogue

"Just as in a hospital, the department of radiology should be a meeting house where different interests converge and draw from each other's store of knowledge to mutual advantage."

A. E. Barclay, 1931[622]

Dr. Barclay's vision of a radiology department as a meeting house sounds very much like the ideal trading zone in which equal partners come together to share their unique perspectives and benefit from each others strengths and special knowledge. But, as we saw in Chapter 1, this ideal trading zone assumes a social and epistemic equality that is not often evident in the actual interactions between different disciplinary cultures. Barclay better captured the situation in radiology when he noted, "The traditional art of medicine has been forced to yield to a science that becomes each day more exact, and more exacting for its practitioners."[623] The desire for cooperation and mutual compromise conflicted with a reality in which increasing authority was awarded to physicists by the radiologists.

The preoccupations of physicists with the nature of radiation, its perplexing classical and quantum properties, were of little use to doctors. Physicists did not give doctors a coherent model of x-ray action on which to base their practice, and in fact such a model did not exist. What they gave them instead was a world-view, a set of values and

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[622] Barclay, "The Dangers of Specialization," 72.

[623] ———, "President's Address: Ideals in Radiology and Electrology," 2.

standards of practice that emphasized precision and objectivity, values that were often at odds with the more pragmatic and individualist culture of medicine.

The adoption of the röntgen as the unit of measure for x-rays in 1928, and the creation of safety guidleines that assumed the existence of one safe dose of x-rays for all bodies, demonstrate the increasing acceptance of the values of physicists' by the doctors practicing radiology. In order to explain this trend, I have looked to the particular social roles taken on by physicists in medicine. In Britain, physicists maintained close contact to doctors, as members of the Röntgen Society, and later, the British Institute of Radiology. They took on leadership roles as physics teachers in medical schools in the early 1900s, and as safety inspectors for hospitals in the 1920s. In the United States, the association of physicists with medicine took longer to develop. Physicists were associate, but not full members of the radiological societies, and were only called on to teach physics in the early 1920s as the first formal training programs for radiologists were developed. Lauriston Taylor at the National Bureau of Standards and George Kaye at the National Physical Laboratory both took on leadership roles, organising research on x-ray safety. However, American physicists were not sent out to do inspections of hospital departments in the 1920s as they were in England. Physicists were not as visible in the American x-ray community, which may begin to explain why the first safety committees and dosage committees were formed later in the United States than in Britain. But in the end, the American community adopted the röntgen and the same set of safety guidelines as the British community.

As we begin to explain the shift in attitudes towards physics in these communities of doctors, we can also look beyond the positions of leadership taken on by individual

physicists in medicine. The increasing authority of these physicists in the British and American radiological communities runs parallel to an increasing reverence for physics in wider culture. The triumphant results of Eddington's solar eclipse expedition in 1919, hailed as confirmation of the revolutionary implications of Einstein's relativity, was one of these moments of widespread cultural interest in physics. And even though the results of these solar eclipse observations had no bearing on radiology, the expedition received a full report in the British *Archives of Radiology and Electrotherapy*.[624]

In a memorial lecture to the British Röntgen Society in 1928, W. Sampson Handley, a surgeon, spoke of the physicist with awe:

> Today the physicist is pre-eminent as the authentic medicine-man of the tribe. His achievements paralyse our wonder, otherwise they would be intolerable … he has asserted his mastery and proved his incredible statements by making the electron the humble servant, messenger, musician, artist and plaything of man.[625]

There is no doubt that the increasing cultural capital of physics had an effect on the dynamics of this one particular inter-professional relationship. These professionals met as experts over specific domains of knowledge, but with already existing attitudes towards the scope of each other's knowledge, attitudes that were shaped, at least in part, by wider cultural discourses.

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[624] S. B., "The New Discovery in Astrophysics," *Archives of Radiology and Electrotherapy* 24(1921).

[625] Handley, "Radiology from a Surgeon's Standpoint," 50..

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